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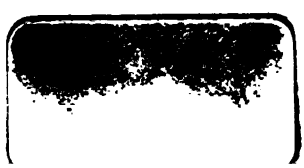

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AND

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# CONTENTS.


PAGE

ACUTE SPLENIC TUMOURS. By Prof. Dr. N. FRIEDRICH, of Heidelberg. Translated by Dr. T. P. SMITH . . . 1

MENSTRUATION AND DYSMENORRHŒA. By Prof. A. GUSSEROW, of Strasburg. Translated by Dr. T. P. SMITH . 28

ON THE CLINICAL RESULTS OF THE LISTER TREATMENT OF WOUNDS, AND ON THE SUBSTITUTION OF SALICYLIC ACID FOR CARBOLIC ACID. By Prof. C. THIERSCH. Translated by Dr. WHITLEY . . . 63

REMARKS UPON EXUDATIONS IN THE NEIGHBOURHOOD OF THE FEMALE GENITAL CANALS. By Prof. OTTO SPIEGELBERG, of Breslau. Translated by Dr. SHEWEN . . . . . 169

THE ACTION OF DIGITALIS. By Prof. TH. ACKERMANN, of Rostock. Translated by Dr. SHEWEN . . . . 179

ON NEURITIS IN RELATION TO ITS DIAGNOSIS AND PATHOLOGY. By Prof. H. NOTHNAGEL, Jena. Translated by Dr. JOSEPH COATS, Lecturer on Pathology in the University of Glasgow . . . . . 201

TRANSFUSION AND AUTO-TRANSFUSION. By Dr. L. LESSER, of Berlin. Translated by Dr. T. P. SMITH . . 237

THE HÆMORRHAGIC INFARCTION OF THE LUNGS. By C. GERHARDT, Wurzburg. Translated by Dr. JOSEPH COATS . . . . . 261

PAGE

THE GENERAL SURGICAL DIAGNOSIS OF TUMOURS. By Prof. A. Lücke, Strassburg. Translated by Dr. Joseph Coats . . . . . 283

RESPIRATORY PARALYSES. By Frank Reigel. Translated by Dr. Shewen . . . . . 309

THE FOOD OF INFANTS. By Prof. F. A. Kehrer, of Giessen. Translated by Dr. T. P. Smith . . . . 346

SYPHILITIC DISEASES OF THE BRAIN AND SPINAL CORD. By Prof. Wunderlich, Leipzig. Translated by Dr. Shewen . . . . . 377

SYNANCHE CONTAGIOSA [DIPHTHERIA]. By Dr. H. Senator, of Berlin. Translated by Dr. T. P. Smith . 403

THE MILDER FORMS OF ABDOMINAL TYPHUS. By Prof. Dr. Jürgensen, Tübingen. Translated by Dr. Whitley 454

ON ARTIFICIAL PREMATURE LABOUR WITH A NARROW PELVIS. By Prof. R. Dohrn, in Marburg. Translated by Dr. Whitley . . . . . 473

ON THE TREATMENT OF DISEASES OF THE STOMACH. By Prof. W. O. Leube, in Jena. Translated by Dr. Whitley . 486

# ACUTE SPLENIC TUMOUR

## AND ITS RELATIONSHIP TO

# THE ACUTE INFECTIOUS DISEASES.

BY

PROF. DR. N. FRIEDREICH,

OF HEIDELBERG.

*A Lecture delivered before the Medical Society of Heidelberg, 12th November, 1873.*

THAT the spleen is liable to alterations, capable of demonstration, in a far greater number of cases than has been hitherto supposed, is a fact which would speedily become recognised if particular attention were to be paid to the condition of this organ in acute disease, and if its clinical examination were conducted as carefully as that of the lungs, heart, liver, bladder, and other organs.

I do not now intend to allude to those recent swellings of the spleen which frequently become developed during the course of various acute primary inflammations of internal organs, as, for example, in pneumonia, pleuritis, &c., and which, as a rule, exhibit only a slight degree of hyperplastic turgescence. Swellings of this kind certainly excite attention on post-mortem examination, but are very rarely discovered at the bedside, on account of the acknowledged difficulty of clinically demonstrating the existence of such trifling enlargements. These instances, however, are sufficient to prove that in the spleen we have to do with an organ composed of elements endued with a particularly high degree of irritability, and one which is prone to react very rapidly when excited, either by simple hyperæmia, fluxion, or the increased temperature of the blood in febrile affections, the result of such reaction being hyperplasia of the splenic pulp.

But, on the other hand, there are numerous acute febrile diseases in which splenic tumours develope to a size far beyond that of those just referred to, and increase to such an extent as to be easily made out by percussion, or even project so much below the ribs as to be clearly recognised on palpation. For such cases as these the assumption of simple fluxion, or the fever-heat of the blood, as exciting causes does not appear sufficient explanation, and we are compelled to seek for other causes to account for such enormous hyperplastic proliferation of the pulpous elements of the spleen. In all acute diseases whose origin is certainly due to infection (typhus, septicæmia, intermittent fever, &c.), the constant occurrence of large splenic tumours can be demonstrated both clinically and pathologically, and hence we are compelled to recognise foreign substances infecting the blood as special irritants of the splenic pulp. It follows, therefore, that the existence of a considerable enlargement of the spleen positively justifies us in forming a decided opinion as to the infectious nature of the malady, and thus, with regard to the origination of doubtful forms of disease, a step will be gained in the still extremely obscure domain of etiology. The readiness with which the spleen reacts to certain substances of an injurious nature, present in the blood, is explained not only by the copiousness of its blood supply, but also by its anatomical peculiarities. We know that the arteries in the interior of the spleen break up into very numerous, extremely fine branches and capillaries, perforated by stomata; that the blood passes from the vessels into wide spaces destitute of walls (intermediary blood canals), from which the veins begin to arise, in like manner, principally as cribriform canals. The blood flows with extreme slowness through these wide intermediary canals, and both here and in the cribriform perforations of the capillaries, is in intimate connection with the lymphoid cells so susceptible to irritation, and the delicate, fibrous network of the pulp. Hence it follows that matters of an injurious nature infecting the blood are extremely liable to be retained in the spleen, to accumulate therein, and to act with peculiar intensity as excitants to the splenic tissue.

Abdominal typhus belongs, as is well known, to those infectious diseases in which large splenic tumours may be very frequently demonstrated. In slight, or even extremely mild forms

of the disease, unmistakable enlargements of the spleen are of regular occurrence, and incidentally, in a discussion which once took place in our Society, with regard to the diagnosis of typhoid fever, we came to an understanding to include in our statistics of the disease, endemic and epidemic in our city, only cases with recent splenic tumours clearly demonstrable; and where the diagnosis was doubtful, to regard the existence of such a tumour as a most essential criterion for the typhoid character of the disease. A very extensive experience permits me to thoroughly confirm Jürgensen's* recent assertions with regard to the significance of splenic tumours for the diagnosis of *typhus levissimus et ambulatorius;* and I have had sufficiently frequent opportunities of demonstrating cases in my clinical wards in which the true typhoid nature of the disease was revealed in an especial manner by the existence of a splenic tumour. But it would be a mistake to suppose that in *typhus levissimus* the splenic tumour is always less in size than in serious cases, or that there is any direct relation between the size of the tumour and the intensity of the other symptoms. A little attention will soon show that, even in the mildest forms of typhoid, it not seldom happens that the splenic swelling attains such a considerable size as to project a long way below the ribs, while, on the other hand, in serious cases the tumour often fails to attain any such dimensions. I have many a time observed most severe cases of ileotyphus, with a constant temperature of 40 C., and even beyond that, in which, during the whole duration of the disease there was only a very moderate amount of the splenic enlargement. Facts such as these prevent us from regarding the size of the splenic tumour as a measure of the intensity of the affection; we must rather take into account the varying grades of resistance (the local constitution) possessed by the splenic elements in individual cases, by reason of which the spleen reacts more or less to the irritation of infectious matter, such reaction being evinced by hyperplasia of the pulp. We may, however, admit that it is in the highest degree exceptional for the spleen to possess such a power of resistance to the irritating properties of typhoid poison as not to become swollen to an extent which can be positively made out; and

* Jürgensen, Ueber die leichteren Formen des Abdominal typhus. Sammlung klin. Vorträge, herausgegeben von Volkmann, No. 61, 1873, p. 489.

granted that a few very rare cases do occur in which, during the whole course of the disease, percussion fails to give positive evidence of the existence of a splenic tumour, these are merely exceptions which by no means negative the possibility of a moderate amount of swelling.

As mentioned in a work of mine published some time ago,* the splenic tumour is usually developed quite at the commencement of abdominal typhus, and, as a rule, forms one of the first symptoms of the disease—a circumstance which also proves that the splenic parenchyma is peculiarly sensitive to the typhoid poison present in the blood. In patients coming under observation as early as the second or third day of the disease, the spleen is often found enlarged to such a degree as to form a very visible prominence below the ribs, and we may thence conjecture that the swelling of the organ may possibly have commenced before the accession of the fever, and the other symptoms of the disease. This supposition has often forced itself upon me, and, by accident, I was able to convert it into a certainty. Several years ago one of my assistant physicians, Dr. v. K. E., informed me that he had discovered a splenic tumour in his own person. An examination was forthwith made, and the correctness of the assertion proved, for I discovered a tumour of such a size that it could be quite distinctly felt projecting below the ribs. My friend appeared in perfect health, and had at no previous time suffered from intermittent fever, or any other disease of which the splenic tumour might have been a possible sequela. The first symptoms of fever appeared a few days afterwards, and resulted in the supervention of particularly severe typhoid, lasting for several weeks. After this had run its course, the spleen returned to its normal size. In this case there was no doubt that during the stage of incubation the spleen had become a large tumour, and this, taken in connection with the facts previously mentioned, that considerable tumours of the spleen can be very frequently demonstrated during the first few days of typhoid fever, entitles me, as I think, to advance the proposition that the commencing typhoid enlargement of the spleen may be, during the stage of incubation, the

* Friedreich, Bericht über 33 im Juliushospitale abgelaufene Fälle von Abdominaltyphus. Verhandlungen der physikalisch-medicinishen Gesellschaft in Würzburg, v. Bd. 1855, p. 285.

first and only effect of the presence of the infectious material in the blood, and that even at this stage the tumour may attain a considerable size. It is true that we cannot at present assert that this condition constantly exists. In those cases, however, in which the splenic tumour becomes evident on percussion only after the appearance of the other symptoms, the possibility cannot be denied that the swelling commenced at an earlier period of infection, but that only after some time had elapsed did it attain a size capable of certain demonstration. Be this as it may, the fact of the occurrence of evident splenic tumour in the incubation stage of ileotyphus is one worthy of notice, and we have here, in my opinion, a means of recognising, in many cases, the presence of infection, even before the accession of the febrile and other symptoms. It might thus be possible, in the case of the inhabitants of a house, or portion of a town, visited by epidemic typhoid, to predict impending disease in individuals from the knowledge obtained from percussion of the spleen—a possibility which might well be immediately attended by practical and therapeutical consequences.

We have seen that in ileotyphus, the spleen is among those organs which are the first to undergo change; moreover, as a general rule, it is the last to return to a normal condition; and I have often demonstrated to my own satisfaction that, particularly in cases where the disease has been of a grave nature, there has scarcely been any diminution in the size of the splenic tumour until convalescence was far advanced. Generally speaking, a long interval has to elapse, after the disease has subsided, before the hyperplastic elements of the splenic tumour undergo solution and absorption, and before nutrition is so far restored that the contractile elements of the splenic tissue and of its vessels can recover their normal tone. I must mention, too, this fact, that, according to my experience, relapses in typhoid cases, which are very much more frequent in Heidelberg than they used to be in Würzburg, are limited in their occurrence to that period of the stage of convalescence during which the splenic tumour is pursuing its tardy course. When once the spleen has returned to its normal size the danger of relapse no longer exists.

In the majority of the other infectious diseases, still less attention has been paid clinically to the condition of the spleen

than has been the case in typhoid. Let us first take diphtheria of the throat, and I must confess myself a decided supporter of the view taken by those who regard the throat affection as a localisation, that is, the local expression of a general infection. This view is positively supported by such cases as I have frequently seen, in which fever and symptoms of severe general disorder have preceded, by one or more days, the appearance of the throat affection. It is true that in pathological records the occurrence of acute splenic tumour as a post-mortem appearance in cases of diphtheria is either not alluded to at all, or only in a very limited number of cases; but this may be explained by the fact that, as a general rule, the tumours in diphtheria are of less dimensions than those in typhoid, and at any rate diminish considerably as death approaches. I cannot, indeed, positively assert that I have always succeeded in demonstrating the existence of a splenic tumour as an invariable clinical symptom in diphtheria of the throat. But of this I can be quite sure, that in the majority of cases, in some even previous to the distinct development of the local affection, there was no difficulty in demonstrating by percussion the existence of a splenic tumour projecting sometimes even below the ribs, and I should venture to look upon this as a proof that the complaint in question was of a primary, generally infectious, nature. There is, however, this difference between the tumours in typhoid and diphtheria, that in the latter case the swelling generally rapidly subsides as the fever and throat affection abate, whereas in the former, as already mentioned, it continues, as a rule, for a long time during the stage of convalescence. In proof of what I have just said, I shall take the liberty of subjoining a brief account of a few cases of diphtheria of the throat occurring in my clinical wards.

1. A woman, aged 30. The disease commenced with shivering, on the 12th August, 1872. On coming into hospital on the 14th August, the spleen was found to be already much enlarged, so as to be felt at the border of the ribs. Febrile symptoms appeared on the 17th August. On the 19th August the splenic swelling was found to be very slight, and it disappeared in the course of a few days. No albuminuria.

2. A journeyman tailor, aged 18. Came to hospital the day after febrile symptoms showed themselves. On the third day of the disease the spleen was seven inches long and four inches wide, and projected below the ribs. On the seventh day of the disease the fever subsided. The spleen began to

diminish on the ninth day, but it was not till the thirteenth day that it had reached its normal size. Albuminuria at no time present during the course of the disease.

3. A journeyman upholsterer, aged 24. The spleen perceptible below the ribs, and on percussion found to be six inches long and three-and-a-half inches wide. On the second day after the subsidence of the fever the spleen began to diminish rapidly, but it only became of normal size after eight days. While the fever was at its height there were traces of albumen in the urine.

4. A baker's boy, aged 16. The spleen on percussion found to be six inches long and four inches wide. No albuminuria. The spleen returned to its normal size not until seven days after the fever had subsided.

With reference to the acute exanthemata, I have met with a series of cases of variola, in which, sometimes even in the precursory stage, the existence of large splenic tumours could be clinically demonstrated. I can thoroughly confirm Birch-Hirschfeld's* assertion with reference to the occurrence of splenic tumours in this disease, but I differ from him in this respect, that whereas he found splenic tumours of considerable size only to occur in the hæmorrhagic forms of variola, to any great extent, I have met with them just as often in the non-hæmorrhagic varieties. In scarlatina and measles Trojanowsky† has reported the occurrence of "not inconsiderable" splenic swellings, but he states that he has found them only in those cases in which a primary incomplete eruption of the exanthem was succeeded in a short time by a second eruption. But according to my experience, splenic tumours, clinically demonstrable, are to be found in the majority of those cases of these diseases which are characterised by regularity both of development and course. Even in scarlatina, I have several times been able to demonstrate clearly the existence of a splenic tumour even during the precursory stage, and certainly in the majority of cases after the appearance of the eruption, and this quite irrespective of the presence or absence of pharyngeal diphtheria as an accompaniment. As to measles, during the present winter (1873-74), in an epidemic which has been raging in our town, I have had repeated opportunities by the bedside

* Birch-Hirschfeld, Der acute Milztumor. Archiv der Heilkunde, xiii. Bd. 1872, s. 411.

† Trojanowsky, Centralblatt f. d. med. Wissenschaften. No. 53, 1871, s. 842.

of satisfactorily convincing myself of the development of acute splenic tumours, the majority of which, it is true, were only of moderate size.

The almost regular occurrence of splenic tumours in cases of facial erysipelas is a matter of peculiar interest. The view has been put forward that the disease in question should always be regarded as of traumatic origin, inasmuch as it always has its starting-point in small wounds and excoriations, especially about the nose. I cannot deny that many cases of facial erysipelas have such a causation; but, on the other hand, we meet with instances sufficiently often in which there is no trace of any lesion, or in which the complaint begins at the upper part of the bridge of the nose, a place where the smallest wound could not possibly be overlooked. In all these cases the theory of a traumatic causation, of an infection proceeding from the integument, must be rejected; it is rather a question of a primary infection of the blood produced in other ways, and of which the affection of the skin, as in the other acute exanthemata, is a consequence and an expression. And with regard to the majority of cases of facial erysipelas, its cyclical course, extending at the most over a limited number of days, reminds us more of the conditions which obtain in the acute exanthemata than of the more protracted course and malignant character of traumatic erysipelas. I am sure, however, that in ordinary facial erysipelas I seldom look in vain for the splenic tumour; indeed, in this complaint, it frequently attains such a size as to project far below the border of the ribs. Sometimes in such cases a simultaneous parenchymatous irritation of the kidneys, caused by the virus present in the blood, is indicated by the occurrence of albuminuria. The best instance of a splenic tumour which came under my notice occurred in a woman who was admitted into my wards on the 11th July, 1872, suffering from facial erysipelas which had extended in an irregular manner over the greater part of the head. This person was the subject of a wandering spleen, so that the displaced organ, very movable and visibly swollen, could be easily grasped by the hands, and its subsequent return to the normal size could be very distinctly made out by palpation. As a rule, I find that these tumours occurring with erysipelas speedily become dispersed as the fever declines, and as the skin affection becomes cured;

and that they do not possess that tenacity which I have mentioned as a peculiarity of the typhoid tumours. It only rarely happens that there is any delay in the resolution process; when this does occur, the tumour may be discovered even when the period of convalescence has somewhat advanced. A case of this kind came under my observation only a short time ago, the patient being a strong maid-servant, 20 years of age. She had had a small superficial wound of the integument of the vertex; this had healed before admission into hospital, but had given rise to erysipelas extending to the face, hairy scalp, neck, and back, as low down as the loins. The disease commenced with high fever, she having been admitted into hospital almost as soon as this appeared. There was then a considerable and unmistakable splenic tumour, which had not completely disappeared even on the fourteenth day after the subsidence of the fever.

It may appear to you at the first glance, Gentlemen, that I am making a somewhat bold assertion, when I say that certain forms of disease, usually regarded as the effects of simple, local irritations, as simple, local inflammations, should be in part transferred to the group of acute infectious disorders. But I trust that the reasons I shall offer will sufficiently justify my proposition. Thus, there is a form of acute coryza, which I am convinced should decidedly be regarded as an infectious disorder. Here we have the development of fever rapidly attaining a considerable height, sometimes with shivering at the onset. The general state of health is much disordered, and the advent of a serious ailment is justly dreaded. Then, on the second or third day, acute coryza appears, which is in no proportion to the intensity of the fever and the general disorder. With the development of the local affection the fever rapidly subsides, and in a few days the complaint is at an end. Physical exploration shows that all the internal organs are normal, only with the exception that a decided splenic tumour can be made out; this, however, rapidly disappears after the subsidence of the fever. We could not possibly suppose that the fever was the consequence of the inflammation of the nasal mucous membrane, inasmuch as the former affection preceded the outbreak of the latter; but as regards its development and course the affection is obviously very analogous to the acute exanthemata, and I think

that from this the infectious nature of the disorder is decidedly apparent, especially if, at the same time, we take into consideration the demonstrable occurrence of splenic swellings at a time anterior to the eruption of the local affection. It may not be without interest if, to exemplify what has been said, I adduce a special case, which, as before, occurred to one of my former assistants. On the 21st of March, 1868, Dr. G. was attacked with sudden, violent fever, headache, loss of appetite, intense faintness, and soreness of the limbs. On the next day a splenic tumour, projecting below the margin of the ribs, could be distinctly made out on palpation. In the night of the 23rd-24th of March violent coryza made its appearance, which was followed by a rapid subsidence of the fever; so that on the evening of the 26th of March the temperature was again normal. On the 27th of March the splenic tumour began to decrease, but it had only completely subsided four days later.*

Just as occurs in some cases of acute coryza, we also sometimes observe peculiar forms of acute angina pharyngea and tonsillaris, which, in my opinion, belong to the class of acute infectious disorders, inasmuch as they originate with high fever, and violent general disorder of a transient character, standing in no sort of proportion to the intensity and trifling extent of the local eruption, and exhibit during their course undoubted swelling of the spleen. I will not trouble you, Gentlemen, with the enumeration of special cases, such as I have frequently observed in my wards, particularly at certain times; for I am convinced that you will find examples of what I have asserted, if you will regularly and attentively examine the condition of the spleen when you are called upon to treat these apparently simple local affections.†

* I subjoin a table of the temperature in this case.

| | Morning. | Evening. | |
|---|---|---|---|
| 21st March ... | — ... | ... 39.5 C. | |
| 22nd March ... | 38.3 ... | ... 39.8 | Splenic tumour evident. |
| 23rd March ... | 38.3 ... | ... 39.5 | Eruption of coryza. |
| 24th March ... | 37.3 ... | ... 38.5 | |
| 25th March ... | 37.5 ... | ... 38.3 | |
| 26th March ... | 37.4 ... | ... 37.0 | |
| 27th March ... | 36.5 ... | ... 36.7 | Commencing disappearance of the splenic tumour. |

† I must take this opportunity of citing a very marked case of acute follicular pharyngeal angina, which only very recently came under my

There are, however, definite forms of acute pneumonia of peculiar importance, which I must decidedly specify as the expression of an infectious disorder, and for which I must claim your attention for a few more moments. These pneumonias which I now have in view differ in many respects, in a very marked manner, from the ordinary, so-called genuine, croupous inflammation of the lungs, such as we frequently see occurring after exposure to cold, especially during the prevalence of keen and bleak north and north-east winds. In the first place the specific course of the local inflammation points to something of a peculiar nature. The ordinary primary pneumonias and pleuro-pneumonias are usually ushered in by shivering fits, and a definite portion of the lung, perhaps a lobe, becomes uniformly and simultaneously inflamed and goes on to hepatisation, which remains confined to the original limits. After a definite number of days, generally from the fifth to the seventh, the fever suddenly abates, and symptoms indicating a crisis appear, while resolution occurs uniformly and almost simultaneously at all points, resembling, in this respect, the advent of the disease. On the other hand, in the pneumonias which we are about to describe, the inflammation, in the first place, begins at a circumscribed spot, and can be easily followed

observation in the hospital wards. G. F., a journeyman tailor, 23 years of age, previously always in good health, was, on the 25th February, taken ill, without any apparent cause, with symptoms of general disorder of system and great exhaustion. As the day proceeded, violent shivering came on, followed by heat and great thirst. These symptoms continued throughout the night, and on the following day violent pains in the head and neck were experienced, and the feeling of general disorder became more intense. There was complete loss of appetite. February 27th, increase of all the symptoms, and, in addition, difficulty of breathing and somnolence. The patient was admitted into hospital on the morning of the 28th February. He had then the expression of a man seriously ill. His face was pale, the eyes languid and dull, there was tendency to somnolence, vertigo, and headache, and intense exhaustion. He complains also of violent pain in the throat on swallowing. The mucous membrane of the soft palate and pharynx generally is of an intense dull red colour, and in the mucous covering of the tonsils and arches of the palate on both sides there are eight or ten follicular abscesses as large as a hempseed. Cervical glands not swollen. Urine scanty, of high specific gravity, and cloudy; contains much albumen. Lungs, heart, and liver normal; the spleen, on the other hand, very much enlarged, to almost double its natural size. Morning temperature 39.2 C., evening 39.4; pulse 100. Ordered a gargle of infusion of sage

by physical examination, as the hepatisation goes on extending day after day to other portions of lung substance. I have seen cases in which the entire lung on one side was affected as just described: the pneumonia commenced in the lower and posterior portion, then spread upwards to the centre, thence to the side, then in an upward direction, and finally to the anterior portions, so that whilst above and in front, at the spots last attacked, there were signs of recent hepatisation, resolution had already taken place at the lower and posterior parts where the disease commenced. This wandering, serpiginous course prevents these remarkable forms of pneumonia from being bound by that limited cycle of time which is observed, as a rule, to characterise the ordinary genuine cases. The fever often continues with unabated intensity for ten, twelve, even fourteen days and more, the crisis does not ensue with the same ease and rapidity, and the transition to defervescence takes place, in the majority of cases, by a *lysis* of some days' duration, and less frequently by such rapid subsidence as marks a crisis. In one case under my notice it was only on the fifteenth day that I observed critical febrile symptoms with copious perspiration and numerous miliary vesicles. On account of the protracted course of the fever, and the slowness with which it

with hydrochloric acid. Internally, acid. hydrochloric dil. 2.0 grammes, aq. distill. 150.0 grammes, syrup. rub. id. 30.0 grammes; a tablespoonful every two hours. Ice poultice to the neck. Antiphlogistic diet. During the course of the following day (March 1) the pains in the throat somewhat abated; the headache became less, and the quantity of the albumen diminished. The splenic tumour remained as yesterday. The temperature now almost normal; in the morning 37.4, pulse 80; evening 37.2, pulse 72. On the 2nd March the follicular abscesses had disappeared; the redness of the throat and the difficulty in swallowing had become much less. The patient feels himself much better. Bowels moved naturally. Urine more copious, no albumen, sp. g. 1010. Splenic tumour decidedly smaller. Morning temperature 36.4, pulse 56; evening temperature 37.2, pulse 64. March 3: patient feels himself well. The throat only very slightly reddened. Appetite returned. Temperature normal. When the patient left the ward quite well, on the 5th of March, the spleen had returned to its normal size. This case occurred at a time when there were several cases of erysipelas and wandering pneumonia in our wards. These latter I shall subsequently allude to. The advent of the fever before the local affection, the severity of the general disorder, out of all proportion to the final symptoms, the albuminuria, and particularly the splenic tumour, are sufficiently definite indications of the infectious nature of the disorder.

reaches its acme, but especially by reason of the decided tendency of the disease to attack both sides, these wandering pneumonias are of a very much more dangerous character than the ordinary forms. Sometimes when the hepatisation is at its height, the occurrence of a more or less marked yellow discoloration of the conjunctiva, delirium, dry tongue, diarrhœa, sometimes of an involuntary character, and alarming collapse, indicates that the disease has assumed a malignant, pernicious character, and causes it to exhibit a certain amount of external resemblance to severe typhoid disorders. But what always appeared to me so peculiarly remarkable in these wandering pneumonias, and confirmatory, more and more, of my conviction as to their infectious nature, was the invariable occurrence of very considerable splenic swellings, demonstrable even in the first few days of the disorder. These tumours in a very short time attained such a size as to form a projection three or four fingers broad below the margin of the ribs, where they could be most distinctly made out on palpation. The size of these tumours reminds one particularly of the typhoid spleens, but, on the other hand, they differ from these, in that after the subsidence of the fever they, as a general rule, rapidly return to their normal size, and, in this respect, are rather to be associated with the conditions obtaining in diphtheria, the acute exanthemata, and erysipelas. But that in such cases we have before us, not simple hyperæmic tumours, the result of passive obstructive engorgement, but hyperplastic tumefactions of the parenchyma, will be perfectly obvious, if we consider that they occur at the very commencement of the disease, at a time when the hepatisation is only slight in extent, and attain dimensions never met with in ordinary pneumonia, or in other local affections, *e.g.* pleuritic exudation, complicated by obstruction in the lesser circulation. But the difference between these serpiginous pneumonias and the ordinary croupous forms is clearly manifest not only in their causation and symptoms, but also in their treatment; and it always seemed to me that the exhibition of quinine and strong, nutritious fluids (soup, eggs, and wine) had a far more favourable effect on the course of these lung inflammations than lowering, antiphlogistic measures.

Many years have elapsed since I first became acquainted with these peculiar wandering pneumonias, and subsequently I

have had almost yearly opportunities in my wards of demonstrating cases of this kind and of drawing attention to their characteristic features. Very recently examples of wandering pneumonia have been described by Waldenburg,* Fischl,† and Weigand,‡ and Kunze also, in the recent edition of his valuable "Lehrbuch der praktischen Medicin" (vol. ii. Part I. Leipzig, 1870, page 150) alludes to this affection in a manner befitting the subject. It appears to me, however, that the above-named observers have failed to notice the occurrence of the splenic tumour, a symptom to which I attach very considerable importance in assigning to those pneumonias a place among the group of the acute infectious disorders. When I connect the peculiar serpiginous course of the pneumonias in question, their marked tendency to affect both lungs (characteristics by which they are allied to the special conditions of erysipelas), with my repeated experience of the frequent occurrence of the lung affection at times when numerous cases of acute erysipelas were coming into hospital,§ the thought always forces itself upon me, whether those wandering pneumonias may be considered as allied to erysipelas in regard to their causation, and may even possibly be identical processes. Many years ago I considered myself justified, from this point of view, in making use, in my clinical lectures, of the term "erysipelatous pneumonia" to designate these cases. From the individual variations of the local constitution of the separate organs, and the greater or less

* Waldenburg, Berliner klinische Wochenschrift. No. 41, 1870.

† Fischl, Prager Vierteljahrschrift. Vol. cxiv., 1872, p. 112.

‡ Weigand, Berliner klinische Wochenschrift. No. 41, 1870; No. I, 1872.

§ Such a period occurred here in Heidelberg in the summer of 1872, in which (June, and especially July) numerous cases of erysipelas of the face and head, acute angina, and wandering pneumonia, in all of which large tumours of the spleen occurred, were admitted into our medical wards. It ought also to be mentioned that at the same time there were several bad cases of traumatic disease among the patients of the surgical division, while in the lying-in charity there were severe cases of puerperal fever, such as had not been seen there for years. At the same time in the clinical wards there were also several cases of intermittent fever, a disease previously of extremely rare occurrence in these parts; and also a few cases of ileotyphus, one of which, complicated with jaundice, and certain peculiarities of course, reminded one strongly of the type called "bilious typhoid." In August, during which we had another case of diphtheria of the throat, and

local morbid diathesis originating therein, we may, to some extent, conclude that one and the same infectious material, dispersed through the whole body, produces sometimes erysipelas of the face, sometimes pneumonia, sometimes inflammation of the pharynx, and so on. But as an effect common to all such cases, as also generally to all infectious diseases, we meet with acute active tumour of the spleen, that being the organ which constantly, and generally at an early period, reacts very sensitively to the infectious material present in the blood.

I feel convinced that a careful consideration of the condition of the spleen would enable us to recognise the infectious nature of many disorders which present themselves as apparently merely local affections, and that the group of the acute infectious diseases would be thereby considerably increased. Continuous clinical observations will, I trust, succeed in bringing forward new materials from which a genetic system of diseases may be constructed, and an etiological basis afforded to therapeutics, a science even at the present day, in many respects, empirical.

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A consideration of the significance of the splenic tumour for the diagnosis of infectious disorders, and of its early development, sometimes going back even to the stage of incubation, leads us to a discussion of the question as to the way in which the tumour can possibly arise in connection with such diseases, a

also one of follicular angina faucium, both exhibiting large splenic tumours, the typhoid fever assumed the dimensions of a very violent but brief epidemic, occurring in a double series; this in the month of September subsided just as rapidly, and subsequently appeared only in isolated cases. At this very moment, while I am writing this essay, my wards contain several cases of erysipelas of the face, among them one having a wandering course over the head, neck, and back, almost down to the loins. Occurring almost at the same time, we had a man-servant, 61 years of age, with wandering pneumonia of the right side, extending downwards and backwards, and then upwards. When the hepatisation had reached the upper portion, there were already signs of resolution in the lower and lateral parts. The last named portions again became affected with hepatisation, and death occurred on the twelfth day of the disease with the supervention of profound coma. When the disease was at its height, purulent pleuritis became developed on the left side, and, at the same time, diffuse purulent pericarditis. There was also erysipelatous inflammation, with subcutaneous purulent effusion, on the left hand and fore-arm.

question most intimately connected with the nature of the agencies of contagion and miasma, and the manner in which the animal organism is affected by them. I am very well aware that, in discussing such subjects as these, I am approaching ground still uncertain, where many roads cross, and where different opinions and sometimes conflicting results of investigations are to be encountered. But the physician may well be allowed to take a survey, from time to time, of the daily increasing range of experimental investigation, and to notice how far and in what way the results obtained are in harmony with the manner of development and course of diseases, when occurring upon a large scale as epidemics, and also with clinical phenomena, as presented to us at the bedside by individual cases.

After the view had been arrived at that an explanation, very far from being unsatisfactory, of the origin and course of the acute infectious disorders is to be found in the assumption of a reception into the body of a simple organic or inorganic virus, the disposition has more and more gained ground to concede the existence of a *substantia viva* (*contagium vivum seu animatum*) as the most probable cause of those affections. There are, in particular, certain low and very minute organisms, some of them scarcely recognisable, comprehended in the group of the scizomycetes (microspores, micrococci, bacteria, bacteridia, vibriones, spirillæ), which are considered to have an intimate connection with the etiology of the acute infectious disorders. Some years have elapsed since Davaine clearly showed that malignant pustule was traceable to infection of the blood by bacteridia, and the continuous prosecution of inquiry at the present time has demonstrated, with more or less evidence, the presence, in the blood and various tissues, of similar low organisms, both in the majority of the other infectious disorders, and also in typhus (abdominal), recurrent fever, diphtheria, pyæmia, septicæmia, the acute exanthemata, erysipelas, &c. It can scarcely be denied that the theory which adopts these low vegetable organisms as the exciting causes of disease gains more and more positive ground as time goes on, or that, unless all signs are fallacious, the investigation is being pursued along the right track.* But, at the same time, we again advance

* Letzerich has recently demonstrated (Berliner klinische Wochenschrift. No. 6, 1874) the vegetable nature of the diphtheritic membrane by its

toward the notion of the old physicians, certainly expressed in a different way, with regard to the parasitic nature of diseases, and the existence of a morbid organism which has invaded the human body, and commenced therewith a struggle for life or death.

Even before general acceptance had been gained by the view which attributed significance to the lowest vegetable organisms for the origination of acute infectious diseases, there was already a tendency, caused by the typical course of the majority of the above affections, to imagine a kind of fermentation, or, at least, an analogous process, which might run its course in the blood. The notion was that some kind of ferment had, in some sort of way, gained admission into the organism, that the effect of this was to excite fermentation, and it was further supposed that the symptoms of disease were the consequence and the expression of the fermentative processes going on in the blood. According to the present state of our knowledge, we might possibly conceive that the schizomycetes are those ferments which excite in the blood a process analogous to that which occurs when a ferment is added to a fermentible substance—for example, yeast to sugar. A closer consideration, however, of this subject soon shows us that a comparison between the conditions existing in fermentation, on the one hand, and the acute infectious disorders on the other, can scarcely be made with success, and that there is no sort of analogy between these respective processes in the way often imagined. The processes of fermentation begin to show themselves in the fermentible substance in a very short time after the addition of the exciting material, and this latter, even when at a minimum, violently accelerates the chemical processes of fermentation, unless, perhaps, substances checking fermentation be added to the mixture. The process of fermentation goes on with continuous rapid increase of the fermentative organisms, until the last remnants of the fermenting substance have completed their chemical metamorphoses. But in acute infectious disorders we very often see an early stagna-

reaction as cellulose to iodine and sulphuric acid. A reaction of the same kind enabled me years ago to demonstrate the vegetable nature of the masses of micrococci, composed of extremely small punctiform elements, frequently found between the epithelial cells of the mouth and throat in the sputa, as well as in the bronchial tubes after death. (Virchow's Archiv, 30 Bd. 1864, p. 394.)

tion and a rapid retrogression of the pathological process, even without our aid (abortive forms), and the symptoms of disease often commence only after a long time has elapsed since infection took place, and after an excessively long stage of incubation. Supposing that we examine the blood, for example, of a man who has died from severe and rapid typhoid, where, it may well be asked, are those chemical and histological changes which might remind us, even distantly, of a process analogous to fermentation? Also, with regard to the other acute infectious disorders, pathological chemistry has, up to the present time, completely failed to prove the occurrence of chemical changes of a peculiar kind in the blood, causing a specific difference between it and the same fluid in other disorders of a febrile though certainly not infectious nature. Without doubt the schizomycetes cannot fail to withdraw from the blood certain substances requisite for their nutrition and their life; but they may do this without producing qualitative changes or profound chemical metamorphoses in the same way in which chemical transformations are observed to take place in fermenting liquids. On these grounds it seems to me impossible to admit that an analogy has been found to hold between the acute infectious disorders and the fermentation processes, as Steudener* has recently attempted to show, and I therefore consider that the term "zymotic diseases," made use of by many authors for designating the acute infectious disorders, is by no means an appropriate one.

The notion has been frequently indulged in that the schizomycetes, having in some way or other obtained entrance into the human body, find therein a suitable nidus for their development, and it has been believed that a certain analogy has been also herein discovered between the acute infectious disorders and the fermentation processes. But no one surely will pretend to assert, that the multiplication of those germs in the organism exhibits even the remotest resemblance to that rapidity and freedom with which organised ferments become developed in fermentible fluids, and that in this respect any great analogy exists. Everyone is familiar with the rapid manner in which the yeast-cells multiply to an enormous extent

* Steudener, Ueber pflanzliche Organismen als Krankheitserreger. Volkmann's Sammlung klinischer Vorträge, No. 38.

in fermenting saccharine solutions; in urine which has undergone alkaline fermentation we notice vibriones and bacteria often in a few hours increasing to such an extent as to cause the fluid to appear very turbid, and to separate therefrom as a thick sediment. The calculation has been made that from a single bacterion, supposing that it doubled itself in an hour, and that each of the two bacteria did likewise in the same time, and that this went on for five days, no obstacle disturbing the advance of the hyperplasia, such a huge mass of these organisms would become developed as would fill the ocean! In view of such proportions can we, for one moment, adhere to the opinion that the schizomycetes find in the human organism a nidus in any degree favourable to their existence? If such were the case, then indeed in every infectious disorder death would invariably occur with the shortest possible notice from thrombosis of all the vessels and infiltration of every organ and tissue with masses of schizomycetes. But, on the contrary, how small is the number of those low organisms which we have been able to demonstrate in the blood and tissues in the acute infectious disorders, *e.g.* typhus, septicæmia, etc.! It is far more evident that the schizomycetes find in the human organism an excessively unfavourable soil for their increase, that their duration of life is here very limited, and that they soon succumb to the resistance which the living organism, the affected blood, opposes to the conditions of their existence and reproductive faculty. They may, it is true, multiply in the organism up to a certain amount, they may even increase to comparatively abundant proportions, but the existence which they henceforth enjoy is only a poor one, and their days are numbered.

The opinion, therefore, cannot possibly be entertained that the course and the symptoms of the acute infectious diseases may be attributed simply to the existence of the schizomycetes in the blood as corporeal particles, or possibly to their physical effects, or the mechanical irritation produced by their movements in the parenchyma of organs. Their minuteness enables these tiny organisms to pass with ease and freedom even through the smallest capillaries, and if occasionally, here or there, they become adherent in a small vessel, and thus form larger masses, local disturbances of the circulation and the attendant consequences would be, at the most, the only result; and, inasmuch

as the organisms are prone to proliferate through the vascular walls at the spots where they have accumulated, the production of circumscribed inflammatory and purulent foci in the neighbouring parenchyma is another possible contingency. The simple existence, however, of the schizomycetes *per se*, even if they should considerably multiply in the blood, will never enable us to explain, even in a tolerably satisfactory manner, either that series of general, severe symptoms of disease, which, even in the absence of such local morbid foci, characterise the acute infectious disorders, or the typical course of these latter, or, lastly, the constancy with which, in single cases of the various infectious disorders, precisely and emphatically the same organs are again and again affected by specific anatomical changes.

If after what has been said we are forced to reject the theory of a fermentation process excited in the blood by the schizomycetes, as likewise the assumption that these organisms, in their capacity as simple corporeal particles, can possibly cause other than merely local disorders, but if, at the same time, we wish to adhere to the notion that the acute infectious disorders originate in contamination of the blood with these low forms of life, it certainly must be admitted that there is a high degree of probability and reason in that theory which attributes a peculiar noxious influence to certain fluid matters, secreted by the schizomycetes during the continuance of their existence in the blood. Consequently, it may be the schizomycetes which primarily infect the blood, but the phenomena of the disease may be due really to the accumulation in the blood of the products of their metamorphoses excreted by them. It is true that this view compels us to admit that those secreted or excreted fluids possess an eminently virulent nature, by reason of which they are enabled to affect most injuriously the functions and nutrition of the organs of the infected body, and this even when they exist in proportionately small quantity corresponding to the relatively trifling amount of schizomycetes demonstrable in the blood and organs in cases of acute infectious disease. The symptoms of disease would therefore have to be regarded as in a certain sense the expression of a poisoning process caused by a fluid virus; but between this and ordinary poisoning by any toxic, organic, or inorganic substance, there

would be this primary difference, that in the latter case, the whole quantity of the poison present in the body reaches it as such from without, whereas, on the other hand, the poison due to the continuous vital activity of the schizomycetes perpetually accumulates in the blood and goes on increasing until their life is exhausted by the resistance offered by the invaded organism, and an end is put to their reproduction. There is, however, a striking harmony in many respects between the symptoms of the acute infectious diseases and those of ordinary poisoning, and a comparison is quite justifiable. I allude here, without reference to the acute splenic tumour, especially to those parenchymatous degenerations of the kidneys, liver, and substance of the heart, etc., as found both in the acute infectious diseases, and in poisoning by phosphorus, arsenic, acids, etc., and which I am inclined to attribute rather to the irritating effects of the poisonous substance circulating in the blood, than to the pernicious action of that fluid at an ardent fever heat.*

If we withdraw our attention from the above-mentioned disorders of nutrition in various internal organs, seen to arise as ordinary consequences of the irritating properties of the many varieties of poisoning under the most diverse conditions of infection, there will yet remain that series of specific diverse anatomical disorders and clinical symptoms whereby the separate forms of infectious disorders are so absolutely distinguished from one another; typhoid, for example, from malignant pustule, erysipelas from diphtheria, scarlet fever from small-pox, etc. Hence it follows that the poisons from which the acute

* This may be the proper place to refer to analogous conditions as we meet with them in trichinosis. Even in this acute infectious disease we must distinguish between the mechanical effects of the parasites (intestinal affections, inflammation of the muscles) and certain other symptoms which doubtless owe their origin to a virulent fluid contained in the capsules of the creatures, and subsequently excreted by the trichinæ after the capsules are dissolved. On a previous occasion (Beobachtungen über Trichinose. Deutches Archiv, für klin. Med. ix. 1872, p. 465) I have thoroughly discussed these conditions, and shown that the initial œdema of the face, the fever, the splenic tumour which often occurs, as also the parenchymatous degenerations of the kidneys, liver, and substance of the heart may be regarded simply as the consequence of irritation caused by that fluid virus. It is well known that the trichinæ themselves never penetrate into the interior of these organs, and the fever is not always so violent as to be capable of producing parenchymatous degeneration.

infectious disorders take their origin must be of a more or less diverse nature, and as we see that the various organic and inorganic poisons, *e.g.* strychnia, morphia, atropia, mercury, lead, etc., although generally diffused throughout the blood, nevertheless affect perniciously only the function and nutrition of single and perfectly distinct organs and tissues of the body, we may venture without hesitation to attribute like peculiarities to the organic poisons excreted by the schizomycetes. But here we are compelled to establish the existence of peculiar kinds of schizomycetes, possessing specific differences for each form of acute infectious disease. Although the schizomycetes found in the majority of the acute infectious disorders appear usually only as simple points, or most minute, staff-shaped formations, yet, from the very various effects which they produce in the human body, it appears exceedingly probable that the morphological identity is only an apparent one, and that improved optical instruments may enable us, at some future time, to recognise differences in the internal organisation of these most minute beings. As a matter of fact, already at the present day observers have succeeded in showing that certain acute infectious diseases are characterised by peculiar, perfectly distinguishable forms of schizomycetes. Thus in malignant pustule the formations are long and staff-shaped (bacteridia, Davaine); in recurrent fever they are corkscrew-like and spiral (spirilla, Obermeyer, Engel). In septicæmia Klebs has discovered a microsporon septicum, and Letzerich has shown that the schizomycetes of diphtheria exhibit important peculiarities of development and manner of reproduction, from which we may venture to infer specific varieties of nature and existence in the lowest forms of animal life.

We can certainly very well imagine that, as a general rule, the schizomycetes gain admission into the organism in a very moderate amount, and that soon after this occurs the poisonous products secreted by them, at first only in correspondingly small quantity, become so much diluted by admixture with the blood as to be incapable as yet of producing any injurious results. The pernicious effects upon the nervous system (fever) and the subsequent specific symptoms of disease will be manifested only at a later period, when the germs have undergone considerable increase in the blood, and the poison thus become

more concentrated. This would account for the stage of incubation characteristic of the acute infectious diseases. In proportion to the difference in the quantity of the germs originally absorbed, and the greater or less resistance offered by the organism in individual cases to their existence, the quantity of the poison will, with more or less rapidity, attain that degree of concentration necessary for the production of symptoms of disease, and this would account for the diversities, within certain limits, in the duration of the incubation stage in particular cases.

The peculiar conditions, already alluded to, of the vascular apparatus and circulation in the spleen would account for the fact that the schizomycetes pass in more slowly and become accumulated and retained in that organ, the result being that the poison soon attains a degree of concentration sufficient to produce irritation and alteration of the pulp, even before the accession of other symptoms of disease. This enables us to understand the constant occurrence of splenic tumours in the acute infectious diseases, and their early commencement, reaching their acme even in the stage of incubation, as I have shown in a previous page to be characteristic of abdominal typhus. This view is supported by the statements of Coze and Feltz,* who, in small-pox, found the largest number of bacteria in the spleen, as well as by the investigations of Birch-Hirschfeld,† from which it likewise appears that in infectious diseases the micrococci particularly accumulate in the spleen, and that they become in part absorbed and retained even by the protoplasm of the cells of the pulp, as also that coloured particles, such as cinnabar, injected into the blood are discoverable in the spleen in great abundance at a very early period, and even at a considerable interval after the injection. In animals that had died from malignant pustule, Grimm‡ found that the enlarged spleen was the principal seat of the bacteria, and says "that we might frequently be led to regard the spleen as composed only of

* Coze et Feltz, Recherches sur les maladies infectieuses. Paris, 1872, p. 197.

† Birch-Hirschfeld, Der acute Milztumor. Archiv der Heilkunde, xiii. vol., 1872, pp. 391 and 404.

‡ Grimm, Zur Pathologie des Milzbrandes. Virch. Archiv, 54 vol., 1872, p. 264.

trabeculæ, bacteria, and their germs." If, as we are justified in assuming from the above facts, the spleen during the subsidence of the acute infectious diseases retains, with peculiar tenacity, the morbid germs, and is the last to get rid of the poison which they excrete, we shall hereby obtain an explanation of the clinical fact that the splenic tumour in the acute infectious diseases is usually the last to undergo resolution, that, as a rule, it outlasts by a greater or less interval the other symptoms, and that, particularly in ileo-typhus, its continuance is often a feature of an advanced stage of convalescence. Moreover, the obstinacy with which the germs remain in the spleen is not without significance in the explanation of the relapses in abdominal typhus, inasmuch as a revival of reproductive faculty in these germs, and consequently a new infection of the entire organism proceeding from the spleen, can certainly not be excluded from our consideration. Clinical experience has long ago proved that relapses in typhoid, apparently of very frequent occurrence here in Heidelburg, take place only during the continuance of the splenic tumour, and that only when the spleen has recovered its normal size, can the convalescent from typhoid be considered safe from any fresh attack. The same holds good with regard to intermittent fever, in which, it is well known, the best way of guarding against relapses is to continue the treatment by quinine, until every remnant of the splenic tumour has finally disappeared.

The period extending from the moment when the poison in the blood attains a degree of concentration requisite for the production of the first febrile symptoms to the time when the last remnants, no longer able to exercise any influence, are extruded from the blood, marks out the duration of the disease. The duration of the disorder, varying within certain limits, in individual cases of the different forms of infectious diseases, may be regarded as the result of two factors; one of which is the quantity of the morbid germs received into the organism, while the other is the sum of the resistances which the latter is able to oppose to the existence and reproductive faculty of the germs in question. It appears to me that the explanation of the variations in the intensity and duration of single cases of disease is to be found in the respective activity of these two variable

factors. The greater the quantity of the germs which have gained access into the body from without, the more intense, *cæteris paribus*, will be the symptoms of disease; the slighter the resistance which the organism is able to oppose to the life of the germs, the more serious will be the danger. If the attacked individual be possessed of considerable power of resistance, and the amount of the infectious germs be but small, so that their life and reproductive faculty rapidly become exhausted, slight and brief disorders (abortive forms) will be the result, or there may even be a complete absence of any of the phenomena of disease in spite of the infection which has taken place. We can thus imagine how it comes to pass, that, for example, during the prevalence of an epidemic of cholera, or typhoid, all those who are subjected to the same pernicious influences, who breathe the same air, drink the same water, etc., do not suffer from the disease in equal degrees of severity, and that some, or even many individuals, although doubtless receiving the morbid germs into their system, remain in perfect health. Infection may occur in all, but disease may ensue only in those cases in which the power of resistance possessed by the invaded organism is relatively or absolutely insufficient to secure the rapid and complete extinction of the parasites. My opinion therefore is that with regard to the acute infectious diseases we ought not to talk of a greater or less individual susceptibility, or the opposite, towards the reception of the morbid germs, the contagium, but of individual differences in power of resistance with which the organism opposes the existence and the multiplication of the germs by which it has become invaded. All those influences which in any way weaken the organism, by no means render it more prone to admit the morbid germs, but they certainly lessen the power of checking their continued existence, and experience has long ago shown that all debilitating agencies (excesses, depressing affections, and the like) are sources of danger, of a very serious kind, to reduced and exhausted constitutions.* In like manner, daily observation has long ago sanctioned the opinion that a most

* I have often observed that persons undergoing a thorough course of antisyphilitic treatment in hospital are prone to be attacked by typhoid. In all these cases the fever is of an extraordinarily severe type and death ensues almost without exception.

important point in the treatment of the acute infectious diseases is to increase the strength and raise the power of resistance and thus gain time, until the morbid germs have lost their power of life and reproduction. Among all the remedies adopted for the treatment of the acute infectious diseases, quinine is the one which has obtained by far the greatest amount of panegyric, and this may be principally attributed to the fact already experimentally proved, that it possesses properties hostile to the existence of the lower vegetable organisms.

It is well known that schizomycetes are able to retain for a long time a latent kind of life, even in the occasional absence of favourable conditions for their development and multiplication, and that they immediately awake, as it were, to a new life with the return of those favourable conditions. We know that bacteria which have been in a desiccated state for some time rapidly again develope when exposed to moist air (Karsten), and also that they are not destroyed when completely frozen at a temperature of – 18°, but that when thawed and placed under otherwise favourable circumstances, they again increase and multiply (Cohn). In the case of putrid infection, Coze and Feltz have shown that even the desiccated and pulverised blood of the infected animals retains its active properties. If to all the forms of schizomycetes originating the acute infectious disorders we were to attribute the faculty possessed by low vegetable organisms of retaining a latent sort of life for some time, and of awakening to renewed activity only on the setting in of peculiar circumstances, this view would, better than any other, enable us to understand the disappearance of epidemics, as well as the recurrence of extinct contagium. The continuity of the contagium, even though it only occasionally attains a condition of activity, would be thereby established, without the necessity of entertaining the theory of an equivocal generation.

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I am perfectly well aware that the doctrine of the significance of low vegetable organisms for the origination of the acute infectious diseases, and of the poisonous products of metamorphosis excreted by them for the causation of the symptoms of disease, still remains strictly within the domain of hypothesis, and that many and manifold reasons may be legiti-

mately urged against it. My primary object has been to apply the test of clinical experience to that doctrine, and to see how far it could be made to harmonise with observations at the bedside. I think that I have shown by the above statements that the symptoms observed during the origination and course of the acute infectious diseases can be better and more completely interpreted by the theory proposed than by any other possible view; and I may also add, that the results of experimental investigation are in part directly favourable to this theory, and, at least, they cannot be cited as stringent evidence against it. Zahn* and Lewitzky† discovered experimentally that the symptoms of disease which followed the introduction of bacteria were due to a poisonous product which they secreted. Orth‡ also agrees in attributing poisonous properties, not to the bacteria themselves, but to their secretion. It was shown by Klebs,§ that while the injection of a fluid abounding in the microsporon septicum caused continued fever, the injection, on the other hand, of the filtrate deprived of the fungi produced merely symptoms of disease of a transient character. In a case of resection of the hip-joint which terminated fatally from pyæmia, Wolff‖ was able to discover masses of bacteria in the secretion of the wound, but none of these organisms in the blood, and in this instance the fatal result might very well have been due to the continuous absorption of the poison constantly secreted afresh by the bacteria in the wound, without the bacteria themselves having gained access to the blood.

* Zahn, Zur Lehre von der Entzündung und Eiterung. Dissert. Bern, 1872.

† Lewitzky, Centralblatt f. d. med. Wissenschaften, No. 46, 1873, p. 723.

‡ Orth, Untersuchungen über Erysipel. Archiv. f. experimentelle Pathologie und Pharmakologie, i. Bd. Leipzig, 1873, p. 81.

§ Klebs, Beiträge zur pathol. Anatomie der Schusswunden. Leipzig, 1872, p. 121.

‖ Wolff, Zur Bacterienfrage. Virch. Archiv, 59 Bd. 1873, p. 145.

# MENSTRUATION AND DYSMENORRHŒA.

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The period of puberty, commencing at ages varying with the mode of life and climate, is characterised by the appearance of a regular sanguineous discharge from the sexual organs, which, occurring at intervals of from three to four weeks, is called menstruation.

This phenomenon, which is almost exclusively confined to the human species, has, since very ancient times, attracted the attention of physicians and the laity. It is not my intention in this brief description to recapitulate all the views and hypotheses which have gained acceptance with regard to this subject, neither do I intend to enter upon the field of statistical investigation, towards which, down to very recent times, a predilection not altogether intelligible has been manifested. We are in the possession of voluminous, by no means uninteresting works, containing most laboriously compiled statistics and averages with regard to the time of the commencement and cessation of menstruation, and its peculiarities in small and large women, in the weakly and robust, and in blondes and brunettes. These are certainly not unworthy objects for scientific investigation, but they are only of subordinate importance as far as regards the comprehension of the whole subject, the more so because the result pretty nearly coincides with what has been known for a very long time: this being, that in the majority of cases menstruation commences in temperate climates between the twelfth and eighteenth year, and continues for a period varying from thirty to thirty-five years. More important,

however, than such calculations as these is the question with reference to the nature and physiological amount of the menstrual discharge. With regard to the last mentioned point, nothing can be asserted positively, for notwithstanding definite statements to the contrary (*conf.* Krieger on "Menstruation"), the determination of the quantity is too uncertain and the individual differences in this respect appear to be too great. On the other hand, certain peculiar properties have long been attributed to the menstrual blood, upon the existence of which certain most extraordinary assumptions have been founded. One of these views attributes poisonous properties to the menstrual blood, etc. It has been observed that menstrual blood coagulates less readily than other blood, it is also said to possess a peculiar odour, but by no means of the same character in all individuals. These and similar observations, accurate in the main as far as they go, are of no particular importance in other respects, inasmuch as the difference between menstrual blood and the menstrual fluid has not been sufficiently attended to. The blood discharged during menstruation differs in no respect from ordinary blood, if obtained free from all extraneous matter, but immediately it escapes from the uterine vessels it becomes mixed with mucus, a larger quantity of which it meets with in the vagina. This secretion having an acid reaction, impedes coagulation, and the peculiar odour already alluded to, which the discharge finally attains, is due to the secretion of the sebaceous glands of the external organs of generation.

Our knowledge with regard to the sources of the menstrual flow is still uncertain. The blood is seen to trickle slowly out of the orifice of the uterus, probably its escape goes on at the same rate as its secretion. If we exclude for the present certain pathological observations (hæmatometra with accumulation in the tubes), the reports of cases of persons who have died during menstruation justify the conclusion that the menstrual flow comes from the uterine mucous membrane, and that menstruation therefore is a function of the uterus the cause of which we have yet to ascertain. On an inverted uterus the blood has been observed to escape from the immediate proximity of the orifices of the utricular glands, apparently in single small drops, which become free on the surface and subsequently coalesce, so that the whole mucous surface is covered with sanguineous

fluid. These observations give us some notion of the manner in which the blood escapes, but we must not forget that the circulation is very much disturbed in a uterus thus pathologically displaced. Generally speaking we have to depend upon the reports of the autopsies of those persons who have died, more or less suddenly, during menstruation. In these cases the results of examination are upon the whole tolerably constant. Virchow found the mucous membrane in a state of hyperæmia, and the blood apparently escaping in single drops. The uterine mucous membrane is succulent and swollen, in a word, its condition is that of acute catarrh. The most recent investigations by Kundrat* on this subject agree in their results with the foregoing, but they are still more minute, and therefore of greater importance. According to his description the mucous membrane of the uterus during menstruation is swollen, from three to six mm. thick, very much relaxed, and soft almost to liquefaction, in addition puffy, and covered with whitish, often bloody, mucus. The membrane is injected, and uniformly reddened. The orifices of the follicles are very distinctly visible, being surrounded by projections of the relaxed stroma, causing them to appear as sunk, funnel-shaped depressions. The turgescence commences slowly to appear before the actual escape of blood, when it reaches its highest point, and subsides as slowly after this has taken place. On microscopical examination Kundrat found fatty degeneration to a slight extent of the cells of the interglandular tissue, these being very turbid, and containing globules of fat. The vessels and epithelial cells of the glands were also found to exhibit this slight degree of fatty degeneration. All these alterations, the swelling, turbidity, and increase of the cells, were confined to the superficial layer of the uterine mucous membrane, the deeper layers containing the bases of the glands appeared normal. In the mucus covering the surface Kundrat found numerous detached epithelial cells.

This description agrees in the main with those of all the earlier observers with regard to the similarity existing between the changes in the mucous membrane of the uterus at the time of menstruation, particularly before the appearance of the flow, and those which attend the formation of the membrana decidua at the commencement of pregnancy, so that for a long time

* Stricker, Medicinische Jahrbücher, 1873, vol. ii.

menstruation has been regarded as pregnancy on a small scale, as a preliminary stage for the development of the ovum. At the outside a graduated difference between the decidua menstrualis and the decidua graviditatis is all that can be demonstrated. This, however, must be borne in mind, that the formation of the decidua, at least the incipient stages of such a process, goes on at intervals of about four weeks, quite independently of impregnation.

The phenomena of menstruation, as just described, were even in early times supposed to have a certain connection with the ovary (Chéreau),* at least as far as the discharge of blood was concerned. This latter was well known not to occur when the ovaries were absent, or in cases where they had been removed; and in those instances in which they formed part of the contents of a hernial sac they were found to be swollen and tender during menstruation. However great was the tendency in former times to connect menstruation with the development of the ovary, Bischoff's discovery of the periodical maturation of ova marked an epoch, and appeared thoroughly to elucidate the connection between menstruation and the previously unknown function of the ovaries. Bischoff's theory has become so completely interwoven with medicine as to require no exhaustive exposition here. What he proved is that at each menstruation a follicle maturates, tumefies, and bursts, that the ovule then escapes and a corpus luteum commences to form. This physiological law must still be considered to hold good, although the process is by no means so clear as would appear to be indicated. On the other hand, we shall endeavour in the following pages to show that up to the present time no facts have been adduced sufficient to upset this doctrine. Although Bischoff also proved that exceptions occurred, for example, that a follicle did not always really burst, and although in particular he expressed himself very carefully with regard to the relationship subsisting between the discharge of blood from the uterus and the processes going on in the ovary, the theory of this connection between menstruation (*i.e.* the escape of blood) and ovulation was very soon formally adopted by Gendrin, Négrier, and others, and the custom at once arose of regarding menstruation and ovulation as the same thing, as involving the same physiological process.

* Mémoires pour servir à l'étude des maladies des ovaires, p. 33.

Bischoff laid great stress on the view that menstruation might be only a symptomatic process of ovulation, that this latter might occur without discharge of blood, and possibly *vice versâ*. In spite of this, however, the relationship between the two phenomena was very soon, as I have said, generally regarded as a causal one, that is, ovulation was considered to be simply the consequence of menstruation, or indeed, it might be the cause. The two processes, however, can never be regarded as cause and effect. The menstrual flow may possibly promote the bursting of the Graafian follicle, but is by no means necessary for its occurrence, and there is still less ground for the notion that the escape of the ovum induces the flow. Whether ovulation and menstruation are the ordinary consequences of one and the same process, a periodically recurring congestion of the genital organs, may remain for the present an open question; at any rate it is certain that ovulation occurs without menstruation, but it is difficult to show that the converse holds good. That ovulation takes place without menstruation is a fact which has been long and universally recognised. The best proof that ovulation has been going on is the occurrence of pregnancy, which has been occasionally found to happen in young girls before the appearance of menstruation; our literature, moreover, abounds with cases of women who have borne several children without ever once having menstruated. There are also very frequent instances of conception occurring in women in whom menstruation had been checked by chlorosis or anæmia; the most common examples of this kind are cases where women suckling children have again become pregnant before the recurrence of menstruation. Lastly, I may mention those rare cases, such as have been observed by Deshayes of Rouen,* in which a woman fifty years old bore children two years after the regular cessation of menstruation.

That menstruation occurs without ovulation is by no means so easy to prove as the converse of the proposition; on the contrary, we must adhere to the statement that regular discharges of blood from the uterus, occurring at equal intervals of time, and recurring through a long period, do not take place unless the ovaries are present. We may therefore assert further that ovaries, and indeed a functionally active state of these

* Gaz. hebd. 14, ix. 1873.

organs, form a necessary part of our idea with regard to the menstrual flow, that according to Bischoff's experiments a regularly recurring menstrual discharge is connected with ovulation, and that therefore this latter is an essential part of menstruation. Many objections have been raised against this, particularly in recent times by Beigel, but they are insufficient, in my opinion, to destroy the truth of the proposition.

In the first place, there have been repeated instances in which a ruptured follicle has been sought for in vain in individuals who have died during menstruation. Kölliker, in examining ten cases where death had thus taken place, found the corpus luteum absent in two. Similar observations have been made by Ashwell, Paget, and others. Apart, however, from the question whether in all these cases the discharge was really a menstrual one, that is, one occurring at least at the proper time, and not of merely accidental origin, the occurrence of similar instances had already been noticed by Bischoff. In these cases we can easily imagine that rupture of the follicle does not take place, possibly because it lies more deeply embedded than usual in the stroma of the ovary, and that it then perishes through progressive metamorphosis. Such instances are to be found at times in the normal ovary according to the observations of Slaviansky, and are to be regarded as physiological.

The view is generally accepted that no ovulation takes place during normal pregnancy, the only anatomical proof of which in the ovary is the formation of a true corpus luteum. This, however, is not always present, and is even said to have been occasionally found where pregnancy was absent.* The peculiar growth, of which the true corpus luteum consists, is to be explained by the great copiousness of the supply of blood to the ovary during pregnancy. If regular ovulation were to take place during the whole of pregnancy we should not be able to understand how it is that all the ruptured Graafian follicles do not undergo the same metamorphosis, and we should expect to find regularly several true corpora lutea. The fact is well known that in many women, during the early months of pregnancy, a discharge occurs very similar to menstruation, in some, indeed, it takes place during the whole duration of pregnancy, and lastly Stein and others have observed that some women have

* *Edin. Med. Journal*, Aug. 1873.

never menstruated except when they were pregnant. All these more or less rare instances cannot at any rate be adduced as proof that menstruation occurs without ovulation, for it has never been clearly specified on what grounds these discharges are to be regarded as menstrual, and in cases where they occur at the usual menstrual periods, there is always the possibility of the occurrence of ovulation. It is at least possible that if such cases were to be closely observed, and an opportunity arose of making a post-mortem examination after delivery had taken place, marks of several corpora lutea would be found, so that cases of this kind would especially support the view that menstruation does not occur without ovulation, at any rate they could not at present be quoted as demonstrative evidence to the contrary.

The doctrine of pseudo-menstruation has led to the greatest confusion in the views held with regard to menstruation. The former term has been applied to hæmorrhages from the uterus, occurring under various circumstances, and characterised anatomically by absence of rupture of a Graafian follicle. If the term menstruation were to be generally applied to these discharges, it would be saying in other words that menstruation was independent of ovulation, that is, that it occurred without it. In these cases, however, the symptoms are at any rate not those of menstruation but principally of uterine hæmorrhage, and the whole subject would be rendered clear if we were to speak of these simply as hæmorrhages, discarding the term pseudo-menstruation. The frequent assertion of aged persons suffering from carcinoma uteri, to the effect that their "courses have re-appeared" after an interval of cessation, is not regarded by the physician as any real evidence of menstruation, and with just as little truth could it be said of hæmorrhage during an attack of cholera or typhoid that menstruation had suddenly appeared at an unusual time. In the meantime we must strictly persist in regarding as menstrual only those uterine discharges which occur regularly at certain intervals, generally at the same time of every month. Post-mortem examination reveals so distinct a difference between the so-called pseudo-menstrual hæmorrhages and menstruation proper, that any comparison between them appears quite out of place. In the pseudo-menstrual processes the uterine mucous membrane, as

has been best described by Virchow, is not only much swollen and hyperæmic, but, as a further point of distinction from the same membrane during menstruation, it presents in a marked degree all the appearances of hæmorrhagic catarrh, inasmuch as blood is found extravasated in the mucous membrane. From simple uterine hæmorrhages these processes can certainly be distinguished by the simultaneous occurrence of changes in the ovaries. These organs are usually traversed by extravasations of blood, which sometimes occur on the surface in the peritoneal covering. Single follicles are found filled with blood. These were not found by Virchow to be ruptured, and this he considers the principal difference between true and false menstruation. These appearances most frequently occur in cholera, typhus (abdominalis), and small-pox; in these diseases they are seen in women who have already reached the climacteric period, and even in little children. Uterine hæmorrhages such as occur in women at the age of puberty are not characterised by the ordinary intervals of menstruation, and are for the most part less copious than the normal discharge.

These hæmorrhages occurring in febrile affections were classed by Gubler* with the bleedings from the nasal mucous membrane, etc., which are seen in the same disorders. The appearances in the ovary show, as already mentioned, that these are not simple uterine hæmorrhages anatomically considered. Perroud† properly draws attention to these cases, and in the course of his work comes to the conclusion that no ovulation occurs in these hæmorrhages, and that they are therefore not to be considered as instances of real menstruation. These processes have been lately studied with the greatest minuteness by Slaviansky in cases of cholera.‡ These investigations prove distinctly that neither the changes in the ovaries nor in the uterine mucous membrane at all resemble those which occur in menstruation, the discharge of blood from the uterus and vagina being the only thing which serves to remind us of that process. Slaviansky consequently entirely rejects the notion of pseudo-menstruation, although in a work which has recently appeared, and to which we shall have to allude, he seems to consider that

* Mém. Soc. biol., 1862.

† Lyon medical, 1871, No. 16.

‡ Archiv f. Gynäkol, vol. iv. p. 285.

these processes influence in a certain way the discharge of ova. Slaviansky found that in cholera, thickening and relaxation of the uterine mucous membrane were almost constant appearances, and that there was also frequently a considerable amount of extravasated blood. These extravasations were found to a varying extent throughout the entire thickness of the mucous membrane, causing superficial destruction; occasionally they were situated in the deeper layers of the mucous membrane, and then produced a partial separation, the membrane remaining apparently normal on the internal surface. It is evident that contractions of the uterus, hæmorrhage, etc., are the necessary results of such conditions. In a few cases, extravasations of blood, smaller in size, were found in the muscular substance of the uterus itself. It is certain that in all these cases the appearances were not those of the menstrual process, but of metrorrhagia, due to acute hæmorrhagic inflammation of the uterine mucous membrane. The various acute morbid conditions of the ovaries found by Slaviansky in typhus, cholera, and septicæmia, correspond to the already recognised type of parenchymatous inflammation of the ovary with extravasation of blood.

It is scarcely necessary to offer any further explanation in order to show that these symptoms which have been unfortunately designated by the term pseudo-menstruation, have nothing whatever to do with the normal process, and that they cannot therefore be adduced as evidence that menstruation occurs without ovulation, the only proof of which would be the regular occurrence of a sanguineous discharge from the uterus in cases where the ovaries are absent. As far as our present knowledge extends, and Puech's* interesting work confirms past experience, it may be considered definitely established that menstruation has never been observed in cases where the ovaries were congenitally deficient or imperfectly developed. On the other hand, cases have certainly occurred in which menstrual discharges have taken place after the removal of both ovaries. These cases have lately been regarded by various authorities as distinctly proving that menstruation has nothing in common with ovulation.

It must, however, be mentioned with regard to this point,

* Puech, Des ovaires de leurs anomalies. Paris, 1873.

that not only where the ovaries are absent or imperfectly developed has menstruation never been observed, but we also find that cases where both ovaries are diseased or have been removed, are characterised, as a general rule, by the cessation of the catamenia.

In recent times exceptions to this rule have been adduced in which menstruation has continued after the removal of both ovaries, or in cases where both these organs were diseased. From these, however, a certain number of cases must be subtracted in which, after the operation had been performed, hæmorrhage from the uterus occurred once or even several times, and never again re-appeared. Spencer Wells, for example, has described such cases, and has considered them as uterine hæmorrhages (epistaxis) and not as instances of menstruation. Cases have also been noticed by Bailly (Traitement des ovariotomisées) in which six weeks after extirpation of both ovaries profuse uterine hæmorrhage took place and recurred after four weeks, but never re-appeared. It was shown by Bailly that the patient was suffering from chronic metritis and erosion causing hæmorrhage. The cases reported by Clay and others are equally insufficient evidence of real menstruation after extirpation of both ovaries, the more so because they were not regarded by the authors as instances of the normal process. Of more importance in this respect are Atlee's * cases. Of those reported by him, under Nos. IV. to VIII., two may be at once struck out as in no way demonstrative. One of these is case V., inasmuch as after extirpation of both ovaries, there were only the molimina menstrualia (pains in the loins and headache) and fluor albus, recurring certainly at monthly intervals. The other is case VIII., in which after extirpation of one ovary the other became diseased, but was not removed, and menstruation went on; this case will be subsequently alluded to. There remain, therefore, three of Atlee's cases in which after the removal of both ovaries menstruation went on regularly for years. A similar case of Jackson's has been communicated by Beigel, in which menstruation went on for twenty months, until the climacteric period was reached. It seems to me that these and similar cases are by no means sufficiently demonstrative to upset the physiological law based on good observations, that

* Diagnosis of Ovarial Tumours. Philadelphia, 1873, p. 35.

menstruation is connected with the functions of the ovaries. Cases in abundance have been reported, and lately most exhaustively discussed by Leopold, in which the catamenia regularly appeared though both ovaries were diseased. For such cases we may assume that those parts of the ovary necessary for its function, *i.e.* those concerned in forming the ova, are still present, an assumption supported by direct microscopical examination, for with tumours of the kind there is often a sufficient remnant of normal ovarian tissue with Graafian follicles; an assumption, moreover, the truth of which is absolutely proved by cases in which both ovaries were diseased, and yet conception occurred, and where, consequently, the production and discharge of ova must still have been going on.* With these cases is connected one of Atlee's, No. VII., in which the patient menstruated regularly after the extirpation of one ovary. The other ovary then became diseased, and was punctured eleven times. The patient became pregnant after the second puncture, and was finally delivered of a living child, three more punctures having been made during the pregnancy. The second ovary was afterwards also removed, and menstruation continued to go on. Atlee's eighth case is also an exception, inasmuch as the patient not only menstruated regularly, but also became pregnant after one ovary had been removed and the other had become diseased. The second ovary was not removed. These cases clearly show how small an amount of ovarian tissue is sufficient for the production of ova, a fact quite intelligible on histological grounds. It seems to me that a consideration of all these cases, extracted from American literature, of menstruation continuing for years after removal of both ovaries, amply justifies the question whether there might not have been some remains of normal ovarian tissue left behind, and which could continue to discharge its peculiar functions. That such a thing might occur certainly seems possible when we consider the often peculiar formation of ovarian tumours and their relations to the broad ligaments, and also the great operative difficulties of ovariotomy so frequently experienced. There are no minute anatomical examinations of the cases above referred to, and with regard to this I may draw

* Spiegelberg, Monatsschr. f. Geburtsh, xxx. p. 380; Leopold, Die soliden Eierstocksgeschwülste, Archiv f. Gynäk, vol. vi. Table ii. Nr. 16.

attention to a remark of Waldeyer's,* who occasionally found a small remnant of normal ovarian parenchyma just where the pedicle of the tumour passed into the wall of the cyst. In one case Waldeyer was even able to demonstrate several well-formed corpora lutea near the insertion of the pedicle, also vesicles which must be regarded as Graafian follicles, certainly without evidence of ova.

In the absence, therefore, of minute investigation into the anatomy and histology of such cases, they cannot be quoted in support of the assertion that there is no kind of connection between ovulation and menstruation. Atlee himself considers that the discharges are a function of the uterine mucous membrane, a customary one, so to speak,—an explanation which is certainly not worthy of the name.

Leopold, in his work already referred to, mentions cases of disease of both ovaries with regular menstruation, in which on examination no normal tissue was said to have been found. Herz's case of sarcoma of both ovaries† cannot however be turned to any account in this respect, for its history is only to this effect: "Mrs. M. S., aged 38, had menstruated regularly since her eighteenth year. She had been confined six times since her twenty-eighth year," etc. The further history of the case—it appears to me to be by no means definitely described—is to the effect that menstruation went on regularly while the ovaries became diseased, at least there is no further stress laid upon this point. Spiegelberg's‡ accounts are of a different kind. He met with two cases of disease of both ovaries with regular menstruation (in one certainly occurring every 14 days), and yet in neither ovary could he discover a Graafian follicle, at least not one in a normal condition. In this case it was not probable that any normal follicles were overlooked, and if so, it is the first in which it was positively proved that regular menstruation can go on at monthly intervals in the absence of any ovarian parenchyma; there was however just this possibility, that the whole tissue of the ovary was not invaded by disease until just after the last appearance of menstruation. Although after what has been said the facts just mentioned do not appear

* Arch. f. Gyn., vol. i.

† Virchow's Arch., Bd. 36.

‡ Monatsschr. f. Geburtsh., Bd. 30, p. 440.

to me sufficient to prove that no kind of connection exists between menstruation and ovulation, it must however certainly be acknowledged that this connection has been by no means clearly or absolutely established, at least that we are unable to point out the way in which it exists.

We can therefore easily understand the attempts recently made to establish more closely the connection between ovulation and menstruation. This question has again been thoroughly discussed by Pflüger. Mainly from theoretical considerations he adopts the view, which, however, the histological investigations of Virchow, Pouchet, and others had already established as a fact, that the change of the uterine mucous membrane in menstruation was the incipient stage of the formation of the decidua. Pflüger's comparison of these processes with the excitement which the surgeon causes in plastic operations does not appear to me to be a peculiarly happy one, for the connection between the ovum and the uterine mucous membrane has no analogy with the adhesive process due to the formation of a cicatrix. More interesting than this idea of the phenomena of menstruation, which in point of fact, as already mentioned, were long ago ascertained, is Pflüger's hypothesis with regard to the connection between ovulation and menstruation. He however very properly insists that a simple causal connection between these two processes does not exist, but his theory is that the constant growth of cells in the ovary is a continually increasing source of excitement to the nerves of the organ. When this stimulus has reached a certain degree of intensity violent congestion of the genital organs occurs from reflex action. This congestion induces on the one hand the menstrual flow, and on the other rupture of a Graafian follicle, so that, according to Pflüger, ovulation and menstruation have one common cause, viz. the congestion of the sexual organs due to reflex action. This hypothesis explains in a peculiarly satisfactory manner the periodicity of the whole process, but is of little value as an explanation in other respects. There are no other facts to support it. The only instance possibly in its favour, in my opinion, is the peculiar condition of menstruation observed in the two Hungarian girls. These two individuals, as is well known, were attached to each other by their backs; they menstruated very frequently at different times, though it was shown

on post-mortem examination that the abdominal vessels of both communicated with each other, and that consequently the circulation was common to both. This case might at any rate prove the dependence of the succession of the phenomena of menstruation upon the ovaries. In the very similar instance of malformation called the "two-headed nightingale," which has lately been exhibited, menstruation occurs simultaneously on both sides. This fact is however without value at present, for it has not been positively ascertained whether this individual has one or two sets of sexual organs. The attempt has recently been made by Beigel to upset the theory of a connection existing between ovulation and menstruation, and Slaviansky's* investigations induce him to adopt the old view that the maturation of ova is not a periodical process, but that the ova are set free by coitus and other conditions of congestion, as, for example, the congestion in menstruation, supposing that at the moment in question a mature follicle is just ready. The characteristic feature of a mature follicle, according to Slaviansky, is that its histological structure is completely defined, and he therefore adopts the view that even in children at a very early age a continuous ripening of single follicles takes place just as in adults. (This, by the way, quite destroys Pflüger's theory with regard to the menstrual discharge.) According to Slaviansky the difference between the maturation of the follicles in children and adults consists in this, that in the former there are never any traces to be found of rupture. The follicles of the child's ovary are deeply embedded in the stroma and are covered by a thick stratum of the cortical layer. In the child the mature follicles perish by retrogressive metamorphosis, rupture takes place only when menstruation occurs, or when the ovary becomes congested from other causes; for instance, during coitus, in febrile diseases or in cholera, just at a time when a follicle happens to be mature. Even in adults the great majority of the follicles perish from atresia and not by rupture. Menstruation is a process entirely independent of the development of the follicle, over which latter process it exercises equally little influence.

Apart from the fact that in this last work of his, Slaviansky absolutely recants that which he had stated to be the difference

* Archives de Physiologie, 1874.

between menstruation and pseudo-menstruation, at least he connects these two processes very closely together, it certainly seems to me that no new facts have been adduced to compel us to return to those old views which have been already controverted by Bischoff, Négrier, Coste, and others. Slaviansky's histological investigations with a view to proving the ripening of the follicle must, as a matter of course, be referred to the more expert, but the question of ovulation not only refers to the ripening of the follicle, but deals more particularly with the rupture thereof, that is, the escape of the ova, which, according to Slaviansky's investigations, is a purely fortuitous occurrence.

Bischoff's theory, by no means as yet set aside, that at the time of each menstruation an ovum escapes from the ovary without any external influence, offers a basis for the most recent, and, as it seems to me, the best supported attempt to explain the connection between ovulation and the menstrual flow.

Soon after the time when the changes in the uterine mucous membrane at the time of menstruation came to be regarded as formation of the decidua, and the slight and gradual difference between the decidua vera and decidua menstrualis came to be recognised, the question arose as to what became of this swollen uterine mucous membrane when impregnation did not take place, when no use was made of the receptacle thus prepared. We know that Pouchet's view was that the swollen mucous membrane was extruded during the interval between two menstruations as a mass of mucus, a view which has never been confirmed by observation. That the mucous membrane would again become swollen was a fact with which inquirers were satisfied. There were certainly occasional expressions of opinion to the effect that the menstrual discharge might possibly be connected with this decay of the decidua menstrualis, in other words that the hæmorrhage from the uterine mucous membrane might be the result of simple escape from the vessels as already described.* (Compare on this subject also Pflüger, *l. c.*) As far

* In an article by Aveling, *Obstetrical Journal*, July, 1874, p. 213, I find the remark that Dr. Power's view is that the menstrual hæmorrhage comes from the raw surface of the uterine mucous membrane after the decay of the membrana decidua. Power's words are, "a woman menstruates because she does not conceive."

as I know, Dr. R. Sigismund is entitled to the credit of having advanced a definite theory, though certainly only in the form of an unproved hypothesis. His view is that the swelling of the uterine mucous membrane and the discharge of the ova are two simultaneous processes independent of each other; that the swelling of the uterine mucous membrane is therefore always the first stage of the gravid state—a view undoubtedly correct, as previously explained—that if the ovum, which has reached the uterus, be not fructified, decay of the mucous membrane by retrogressive metamorphosis is the result, the membrane being thrown off as detritus, and this process going on in the uterine mucous membrane causes injury to numerous vessels, and thus produces the menstrual discharge.* According to Sigismund, therefore, the menstrual hæmorrhage is a sign that the ovum of that period has perished. This view at first appeared extremely well adapted for the explanation of numerous difficulties with reference to the relationship of ovulation to menstruation; but the facts in support of it were too few, it was too hypothetical to meet at once with general approbation, although there are but few objections which can be brought against it.

Löwenhardt,† in his investigations with regard to the duration of pregnancy, has confined himself to the examination of actual facts, for he has endeavoured to show that in reckoning pregnancy we must not take as its starting-point the last occurrence of menstruation, but that the ovum of that menstrual period which failed to occur is always the one which becomes fructified. According to his calculations impregnation occurs before the expected commencement of the menstrual flow, consequently before the hæmorrhage should show itself, and, perhaps, from five to eight days before the next expected period. The ovum then fructified reaches the uterus, where the mucous membrane is already becoming swollen, and the decidua in process of formation, and where nothing opposes its further development. The uterine mucous membrane goes on swelling, does not bleed —menstruation fails to appear. It would take me too far from my subject to recapitulate all the calculations which have led to this view, or to illustrate them by observations of my own; these I reserve for another opportunity. Reichert has also

* Berliner klin. Wochenschr., 1871, No. 52.

† Archiv f. Gynäkol, Bd. iii.

expressed the same views with regard to these conditions with particular clearness, and in a very convincing manner. (Description of an Aborted Ovum, etc., "Transactions of the Berlin Academy of Sciences," 1873.)

In fruitful menstruation there are, according to Reichert's explanation, two primarily essential acts, the escape of a mature ovum, and the formation of the decidua menstrualis, as the introduction and preparation of the womb for the formation of the decidua vera and reflexa (Fruchtkapsel). Now if the ovule which has been set free becomes fructified only after hæmorrhage has taken place, consequently, after the appearance of menstruation, as is commonly supposed, this must wait for its further development until the next formation of the decidua, that is, until the next menstrual period. Therefore, as Reichert very properly urges, two successive menstrual periods are required for the process of propagation to become complete, one supplies the ovum, the second provides for its incapsulation, and therefore in the last, the ovum, and in the first, the decidua menstrualis is wholly superfluous. If, on the contrary, the theory be adopted that the ovum which escapes at the time of menstruation then and there becomes fructified, and, as a result, the decidua menstrualis becomes perfected and transformed into the decidua vera, causing the menstrual flow to fail, it follows that only one menstruation period is necessary for the process of propagation. The menstrual discharge would, therefore, only occur when the ovum has not been fructified. The opinion formerly held, and, strictly speaking, the one which now obtains, that the menstrual flow is a sign of ovulation, and that after this had occurred fructification of the ovum could take place, assumes the truth of the theory described by Löwenhardt as preposterous, that the changes in the uterine mucous membrane, the formation of the decidua, must be repeated eight or ten days after menstruation if fructification occurs, for it is a well-known fact that all signs of the decidua menstrualis disappear almost immediately after hæmorrhage has taken place. Reichert thus definitely describes the result of his embryological investigations:—"That the escape of mature ova, and the preparation of the uterus by means of the decidua menstrualis for the incapsulation of the fœtus, take place only during the period of menstruation, and that the incapsulation of the fœtus depends upon a

definite state of its formation, and, as a consequence thereof, invariably occurs at the expiration of a regular period of time after previous impregnation of the ovum." This condition can only be fulfilled if the completion of propagation is connected with a menstrual period, as above explained. This theory of the relations existing between ovulation and menstruation, or rather the menstrual discharge, is supported in the most striking manner by Kundrat's recent anatomical investigations with regard to the menstrual process (Kundrat and G. I. Engelmann: Untersuchungen über die Uterusschleimhaut. Medicinische Jahrbücher von Stricker. Wien, 1873). We have already alluded to the results of these histological investigations into the structure of the uterine mucous membrane during menstruation, and they show that acute swelling takes place, that the membrane, in fact, is in a state of catarrh. When hæmorrhage occurs there is fatty degeneration of a slight degree of the superficial layers, and the secretion contains numerous detached epithelial cells, portions of the orifices of the glands, etc. These changes are not independent of the hæmorrhage, but they are really the cause of it. The destruction of the superficial layer of the uterine mucous membrane produces a raw surface, and hæmorrhage is the consequence. The menstrual congestion by itself only tends to the formation of the decidua menstrualis, the destruction of the decidua causes the hæmorrhage. Analogous to this are the conditions of the decidua during pregnancy, where the intense hyperæmia is not in itself found to produce hæmorrhage, this occurring only at the termination of the period, when delivery takes place, and being due to fatty degeneration of the decidua. In menstruation the blood is only found superficially upon the mucous membrane of the uterus, corresponding to the fatty degeneration in the upper layers in which a large portion of the epithelium of the glands and surface undergoes disintegration. If the hæmorrhage were the direct result of the hyperæmia, extravasations of blood in the deep layers of the decidua would be of much more frequent occurrence. Kundrat agrees with Sigismund and Reichert in the opinion, that according to the received theory which supposes that the bleeding takes place simultaneously with, or as the cause of, the ejection of the ovum, it would be difficult to suppose how this latter could become impregnated and fixed

when the mucous membrane was partially disintegrated, a theory the difficulties of which are not removed by Pflüger's illustration borrowed from surgical processes for purposes of inoculation. The result of histological examination is the establishment of the fact of the retrograde metamorphosis of the decidua menstrualis during and after the hæmorrhage, and it is only slightly probable, at any rate not proved, that a second formation of the decidua should be caused by conception. There is no difference between the decidua menstrualis and the uterine mucous membrane at the commencement of pregnancy. The menstrual swelling is, therefore, only the preparation for conception, the escape of the ovum is subsequent to the swelling, and always precedes the menstrual flow, the occurrence of which is prevented by conception, for when that takes place the mucous membrane undergoes further development instead of disintegration.

The disintegration of the decidua menstrualis being thus anatomically demonstrated, all objections against the hæmorrhage being regarded as the consequence of this process are therefore invalidated.

Hausmann's opinion was that the menstrual fluid contained too few elements to indicate retrogressive metamorphosis of the decidua. He found it to contain cylindrical epithelium of the uterine mucous membrane and glands, and this proves the correctness of Kundrat's observations as to the superficial disintegration of the mucous membrane, the deeper layers (glandular layer, Friedländer) of which remain intact.* Pathological conditions, such as occur in dysmenorrhœa membranacea or extra-uterine pregnancy, cannot be brought forward as evidence in any way against these observations, and the more so because, on the whole, the condition of the uterine mucous membrane in these affections is quite in accordance with what this theory would lead us to expect. A further objection is deduced from the phenomena of heat or rut in certain animals, a condition in which, as is well known, impregnation occurs, and which is not seldom characterised by the occurrence of a bloody mucous discharge from the genital organs. But apart

* Nielander also found that the menstrual fluid contained the elements of the uterine mucous membrane, masses of cells corresponding to the glands (Solowieff, *l. c.*).

from the fact that these conditions have not been sufficiently investigated to admit of any comparison with menstruation, these animals (bitches and cows) have, as Reichert (*l. c.*) pointed out, no decidua menstrualis, and it has not been proved whether or not the bloody mucus comes from the points of attachment of the placenta.

All the foregoing statements may be thus summed up in a few words. There is no possible doubt as to the correctness of the theory of the periodical maturation of ova in the human ovary; and we know that each menstrual period, that is, each ovulation is accompanied by a formation of decidua in the uterus, this latter process not being dependent upon the ovulation, no demonstrable connection, at least, existing between them.

Reichert's careful investigations (*l. c.* p. 9) make it probable that extrusion of the ovum takes place only after the formation of the decidua menstrualis, and that the menstrual flow only occurs when the liberated ovum is not impregnated.

The connection between the menstrual flow and ovulation would consequently be that the advent of the former indicates that the latter has taken place, but that the ovum has not been impregnated.

Menstruation fails to occur when the ovum has been impregnated, its appearance being due to superficial disintegration of the decidua in the absence of impregnation.

It would take me too long to show how admirably this theory of the processes of menstruation is adapted to remove many of the difficulties already described which beset the commonly received opinions on this subject. As regards practice, this theory will of necessity cause some change in our present method of computing the time of pregnancy, whilst on the other hand, the certainty of the calculation will afford a test of the correctness of the theory. These are points, the complete discussion of which I must reserve for another opportunity.

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In dealing with the second portion of my subject, viz. those disorders of the menstrual processes to which the term Dysmenorrhœa is applied, I shall follow the custom which indi-

cates how prone we are, even in the present state of our knowledge, to regard symptoms rather than the actual nature of the disease. Painful menstruation can, as a matter of course, only be a symptom of some kind of disorder in some portion of the genital canal; we therefore apply this term substantially to those painful attacks occurring during menstruation, the causes of which are not readily obvious, excluding from the category all decided forms of internal disease, such as are due to inflammation, change of position, etc. If we thus limit the use of the term Dysmenorrhœa, and, as formerly, consider it to be an independent affection, we shall find that two characteristics are, almost without exception, common to all the cases, notwithstanding the infinite varieties of exciting causes. The first of these is that the dysmenorrhœic troubles are most violent and of most frequent occurrence in young people at the commencement of puberty, and that, therefore, the number of those patients in whom dysmenorrhœa comes on later, or is of long duration, is very small indeed in comparison with the number of those who suffer from violent attacks of pain in the first years of menstruation, and which gradually subside as time goes on. To this we may add, that this form of dysmenorrhœa always seems to pass away after conception and delivery. The other symptom common to all is that the attacks of pain not only commence, but even reach their acme before the discharge appears, so that with the hæmorrhage (menstruation) there comes the feeling of relief. We shall see that an explanation of these symptoms is to be found in the nature of the various forms of dysmenorrhœa, and that all these cases are due to faulty development of the genital organs, and that they may or may not be associated with defective development of the entire organism.

In order to understand these conditions and their therapeutics, it is important to distinguish two principal forms. In the first class will come those cases of dysmenorrhœa in which ovulation is painful, and all the symptoms originate from the ovary; these cases may be designated as ovarian dysmenorrhœa. The second class comprehends those cases in which the excretion, the discharge of blood from the uterus is the principal cause of the pain; such cases may be best described as uterine dysmenorrhœa.

The first group, that comprising ovarian dysmenorrhœa or painful ovulation, is best illustrated, as it were experimentally, by those cases of defective development of the uterus in which that organ is without a cavity, and menstruation consequently fails to occur, but where the woman is of full development and where the existence of the ovaries can be satisfactorily demonstrated. Cases of the kind are not rare, and we generally find that such patients suffer from most violent attacks of pain, at intervals of three or four weeks, in the lower part of the pelvis, the pain more or less resembling that of peritonitis, and associated with vomiting, the subsequent attacks being often complicated by febrile symptoms, and exudations in the pelvic cavity being often discovered on examination per vaginam or rectum. Then we frequently have the occurrence of all those symptoms known as Pelveo-peritonitis hæmorrhagica, and these are very violent, resembling more or less completely those known as colica scortorum (colic of prostitutes). The difference between the two sets of symptoms is, that in the malformation before us the symptoms of inflammation in the pouch of Douglas are primarily due to the menstrual swelling of the ovary, there being no means of depletion by the usual channel, that is, the menstrual discharge, the usual consequences being inflammatory conditions of the ovary, oophoritis and perioophoritis. Then comes the rupture of a vesicle in the ovary in a state of chronic inflammation, and, as a very probable consequence, extravasation of blood into Douglas' pouch. The symptoms thus become more and more aggravated until they lead to general peritonitis, or until the ovary imbedded in masses of exudation matter shrivels up and perishes, and the production of ova ceases. In colica scortorum the symptoms have an opposite course. Pelveo-peritonitis, with subsequent agglutination of the pelvic organs to each other and displacement of the ovaries, etc., is produced by the mechanical injuries, which are not wanting even during menstruation, and the noxious influences of the most diverse kinds to which these women are at this time exposed. This offers a ready explanation of the attacks of pain occurring at each menstruation, and also of the well-known sterility of common prostitutes. The treatment of these dysmenorrhœal attacks due to imperfections of the uterus can, as a matter of course, be only a symptomatic one,

unless we adopt the plan recommended by R. Battey, of Atlanta, Georgia,* and extirpate the ovaries, and thus remove the cause of the affection. Such an operation might be had recourse to in cases of this kind, which are certainly of rare occurrence. In cases where the uterus is defective, that is, rudimentary, the ovaries are at any rate completely superfluous organs, and their removal is of no consequence. Our present experience teaches us that, owing to the smallness of the wound, the removal of both healthy ovaries is a procedure attended by no danger, as compared with the suffering and risk which such a malformation must cause. Practically, however, considerable difficulty will be experienced in choosing the time for this operation; if we wait until there have been repeated and severe attacks of pelveo-peritonitis, the removal of the ovary from the surrounding masses of exudation will be both difficult and dangerous. If, on the other hand, the symptoms are less intense, there will be the less reason for so serious an operation, for, as is well known, it often happens that imperfect formation of the ovary is associated with rudimentary development of the uterus.

In this class of ovarian dysmenorrhœa must also be placed a large proportion of those menstrual disorders so excessively common at the commencement of puberty. The more or less intense pains, which so many young girls experience every time menstruation occurs, not only during the first months, but even for some years, usually before the discharge appears, or during its continuance, for the most part evidently on both sides of the uterus and extending from the loins to the thighs, associated with peritoneal irritation and vomiting—these pains in cases where the position of the uterus is normal have always been referred to the ovaries. The theory is that the commencement of ovulation is attended with difficulties by reason of the peculiar closeness of the stroma of the ovary or of the tunica albuginea. Inasmuch as the surface of the ovary is only partially covered by peritoneum, we must reject at once, as pure hypothesis, the idea that this investment possesses such an amount of consistence. There is much more reason for adopting the theory, based upon recent histological investigations, that in the cases before us, the single follicles are at a distance from

* Atlanta Medical and Surgical Journal, September, 1872.

the surface of the ovary, deeply imbedded in the stroma, or that the tunica fibrosa of single follicles is abnormally closely connected with the wall of the follicle, for, as is well known, the older follicles much more readily shell out of the stroma of the ovary than those of more recent origin. If we suppose this close tissue in the state of menstrual congestion we can easily understand the violent pains as a consequence of the dragging and tension, and also how it is that the depletion, the hæmorrhage from the uterus, partly relieves the symptoms. This theory then serves to explain the ordinary course of the symptoms. As a consequence of the repeated recurrence of the menstrual congestion, the resistance offered by the structures of the ovary is gradually overcome, ovulation goes on without difficulty and without pain; or if congestion is again set up, as it frequently is by coitus, the catamenia occur with rapidity and in a striking amount, and the marriage of the patients is found to cure their dysmenorrhœa. I must, however, expressly remark that I am now considering only those rarer cases of this kind where there is no abnormal condition of the uterus. However, if the symptoms just described become more intense at the period of ovulation, the difficulty perhaps especially severe, we then may expect the advent of really inflammatory conditions in the pelvis, perioophoritis with all its symptoms, and the consequences thereof may be, in a more or less marked degree, those troubles which we have above described as characteristic of ovarian dysmenorrhœa in cases of defective formation of the uterus. These pelveo-peritonitic adhesions and callosities which may form round the ovaries undergo resolution in favourable cases, but, unless this occurs, as a matter of course they increase the troubles in every subsequent menstruation, which is also injuriously affected by everything that produces congestion, especially by coitus, and in these cases we are very often called upon to treat more or less violent attacks of localised peritonitis occurring soon after marriage, and the consequence of which is often incurable sterility. In the description of this group of ovarian dysmenorrhœas I have considered only those cases in which the genital organs, and particularly the ovaries, exhibit no pathological defect, that is, where there are no indications of any original imperfection in the formation of these organs. The difference between these cases and those above described is

often very remarkable. Menstruation is irregular and imperfect, it either does not occur at all, or only in very slight amount at long intervals, or perhaps it may show itself a few times at the age of puberty and then finally cease. In the great majority of these cases, however, according to my experience, the menstruation is unattended by pain, or if any troubles do occur they are slight in character. The individuals are in general of a pronounced type of womanhood, but the genital organs are often remarkably backward in development, the labia majora and minora appearing as small folds of skin, the orifice of the vagina patulous, and the mons Veneris showing only a few hairs. The vaginal portion of the uterus is small, button-shaped, and short, the orifice being scarcely perceptible; its small size is easily discoverable on examination as well as by sounding, while, generally speaking, the ovaries are not to be felt, the difficulty being increased by the amount of adipose tissue which these individuals have a tendency to accumulate. This tendency often causes the breasts to appear fully developed, whereas the proper glandular parenchyma is present in very small quantity. This tendency to the formation of fat is a characteristic distinguishing mark between these persons and women of a masculine type, in whom as a rule the genital organs are quite normally developed. Whilst therefore defective development of the ovary is usually connected with a similar condition of the genital organs, the possible explanation of the rare occurrence of the violent dysmenorrhœas may be that there are here no great impediments or resistance to be overcome; all the functions of ovulation and menstruation are feebly performed, or remain in abeyance in consequence of the defective development.

If, in this way, by analysing separate cases, the attempt be made to gain an insight into, or perhaps only an idea of, the multitudinous varieties of the conditions of dysmenorrhœa, the experience of every physician will remind him of another and a numerous group of cases, which to some extent may be placed in the class of ovarian dysmenorrhœas; I refer to the disorders of menstruation in chlorotic patients. Although the connection between chlorosis and disorders of menstruation was long ago recognised, our knowledge with regard to it is still far from definite, and it must yet be studied more minutely. In those

chlorotic conditions of a transient character occurring in early life, there may probably be only some defective formation of blood, a peculiar form of anæmia, although we have absolutely no certain chemical or physiological knowledge with regard to this. The fact that in a large number of chlorotic individuals the peculiar symptoms disappear as time goes on may be mentioned as supporting this view. In these patients we most frequently observe that menstruation fails to appear, or that it is very irregular, but there may be a complete absence of any painful sensations. Dysmenorrhœa proper is, however, not very rare, and may certainly exhibit all the above described characteristic features. In these cases we may suppose that the ovaries are in this condition, namely, that their tissue is in other respects normal, and that there is no increase of resistance, but that the menstrual congestion itself is insufficient. At any rate, in the absence of any further complications, which is certainly the case as a rule, we see that as the chlorosis disappears, the menstrual processes cease to be painful.

Much more difficult to comprehend are those cases of profuse menstruation in chlorotic individuals, for they are unaccompanied by pain, but, according to my experience, only where there is no displacement of the uterus; these are, however, beyond the compass of our present examination. There now remains a second group of chlorotic cases, in which the symptoms and consequences of the chlorosis are permanent and incurable. The nature of these cases has been made intelligible and clear by Virchow's work on the connection between chlorosis and defective vascular development. As, according to Virchow, defective development of the genital organs is by no means always connected with this vascular hypoplasia, yet both conditions are sometimes found together, and in this case dysmenorrhœa is never absent. In a practical point of view it is at any rate important to recognise these cases, for it is vain to expect any good results from our therapeutical efforts, either as regards the chlorosis or the dysmenorrhœa.

In this last respect, however, mistakes have been especially frequently made. In these cases, that is to say, there is defective development, not only of the ovaries, but also, as a general rule, of the uterus. When this latter organ is formed as in the infant, its mouth is naturally small and narrow. If attention

be paid exclusively to this prominent symptom, the narrowness of the orifice of the uterus, and it be considered as the cause of the dysmenorrhœa, division of the orifice will then be recommended, but it will be a very unnecessary operation, and attended with no good results.

The principal points to be attended to in the treatment of the different forms of ovarian dysmenorrhœa will, after the description given above, be as follows. In cases where increase of ovarian congestion appears indicated, in order to promote ovulation, we shall have recourse to warm sitz baths, warm fomentations and vaginal injections, if possible. In addition, hot foot-baths, dry or wet cupping to the loins, perhaps also occasionally leeches to the inner surface of the thighs will all be useful. In all these cases care must also be taken that the bowels are thoroughly emptied both before and during menstruation, and we may in addition use the clysters with extract of aloes recommended by Schönlein. In making these attempts, however, to increase local congestion, we must be very careful not to forget that this is just the form of dysmenorrhœa which is so prone to cause pelveo-peritonitis, and should symptoms of this affection occur they must be treated according to circumstances by hydropathic measures, or quite cold fomentations, the administration of ice, opiates, and so forth. Opium or chloral in clysters are the best remedies for soothing the pain. If attacks of this kind have once occurred, we must never be induced, when the menstrual period comes round again, to have recourse to stimulant remedies or to the so-called emmenagogues.

In those forms of chlorosis which appear amenable to therapeutical treatment, we may have recourse to the usual measures. The diet and manner of living should be carefully regulated; when possible the cold water cure may be had recourse to, with due precautions, or we may advise sea bathing, or a residence near the sea, or in an elevated spot. I have seen more good results from such means as those than from a methodical course of iron.

If we now turn to the second principal class of dysmenorrhœa which assumes a prominent place after the development of puberty and includes those cases in which the cause of the pain is in the uterus itself,—cases of uterine dysmenorrhœa or of painful menstruation in the strict sense of the words,—we shall find that there are two particular factors to be attended to. In

the first place a large proportion of these cases of dysmenorrhœa are due to anteversion and anteflexion of the uterus, conditions which, as is well known, are often congenital, the uterus having more or less definitely retained its original position and infantile form from the time of its embryonic development. It would lead us too far from our subject to enter more minutely into these congenital anteversions of the uterus so frequently met with, and I will only remark that the symptoms are essentially different according as defective development of the genital apparatus is, or is not associated with the changes of position so frequently occurring. In the first case all symptoms of dysmenorrhœa are often absent, as we have already previously noticed, and such cases come under the cognizance of the physician only because the individuals after marriage are found to be sterile, or because some inflammatory affections have supervened. The symptoms are different when the uterus is tightly and firmly fixed to the posterior walls of the bladder, its development, like that of the ovaries, being normal; in such cases there is nothing more common than violent dysmenorrhœa. While in the first class of cases, it not seldom happens that as a consequence of the menstrual congestion or of the regular performance of coitus after marriage, the uterus, as it appears, gradually becomes developed, and its position becomes restored, these signs of amelioration very seldom occur in the latter group, and in these cases the troubles often considerably increase as time goes on. These are, moreover, just those cases of anteflexion, in which I have seen great benefit result from the use of an intra-uterine stem, when this appliance can be worn.

Finally, as a consequence of defective development of the uterus, or at any rate, of a part of this organ, we must mention that form of dysmenorrhœa, which in recent times has been more and more recognised as the essential type of diseases due to a mechanical cause—I refer to that form of dysmenorrhœa which is never absent where there is stenosis of the canal of the cervix. This stenosis is nothing but a defect in the original formation of this canal. In cases of this kind we find, in the first place, that the uterus is of normal size (the hypertrophies, as we shall see, are often a consequence), the vaginal portion sometimes, but by no means always, small and delicate, the external orifice is quite abnormally small, being often only a

small depression of the size of a pin's-head, which often takes some time to find. In the most marked cases, it is very difficult to introduce even a piece of fine wire, and this contracted condition always extends to the whole of the canal of the cervix, so that as far as the internal orifice, there is the same difficulty and resistance to be encountered in introducing a fine probe. The diagnosis therefore of the condition in question is readily made, the naked eye is able to recognise it, probing, that is, the attempt to do so renders the diagnosis certain. Congenital stenoses of the internal orifice, which always forms the narrowest portion of the canal of the cervix, may occasionally occur, but I have never been able positively to convince myself of their existence, and a perfectly exact diagnosis thereof is, I am sure, very difficult, if not impossible. The stenoses of the internal orifice occurring in retroflexion and anteflexion of the uterus are the simplest and most easily made out, and they cause the mechanical dysmenorrhœa which exists under these circumstances; this, however, belongs to the symptoms of displacements of the uterus.

In the diagnosis of congenital narrowing of the cervical canal, my experience teaches me, as I have before said, that a sufficient distinction has not been made with regard to those cases in which this stenosis is only one of the symptoms of general defective development of the uterus, and is present as a matter of course, and can be designated neither as a peculiar pathological appearance, nor regarded as an object for treatment. We shall see that this circumstance has given rise to much misunderstanding in the views with regard to the significance of limited stenoses of the cervical canal.

When the functions of the uterus and ovaries are normally discharged, the symptoms caused by the stenosis of the cervical canal are always of the same character. Before any visible discharge of blood (menstruation) occurs, the patients suffer from pain, which gradually increases until it becomes intense, usually radiating from the loins, and having a close resemblance to the pains of uterine contraction. When this pain has reached its acme, discharge of blood takes place, and with this there is often complete relief of all the symptoms. It not very seldom happens that these painful sensations are several times experienced during one menstruation, attacks as described again and again

recur, and are followed by a greater or less increase in the quantity of blood discharged. In other cases the symptoms begin with the escape of a small quantity of blood without much pain, then suddenly violent uterine colic sets in, and when this is at its height a quantity of coagulated blood is usually expelled. With these symptoms other attacks of the most diverse kinds, such as vomiting, etc., are often associated, but these may be regarded simply as consensual phenomena. The essential nature of the whole of this dysmenorrhœal process is easily explained by the stenosis of the canal of the cervix. If, as above described, we must assume that, in menstruation, the blood drains away just as it escapes from the surface of the uterine wall, any obstacle to its passage will favour, and will be certainly followed by, its accumulation in the cavity of the uterus. An accumulation of this kind, however, will expand the uterus, that is, will force the uterine walls apart, these having been previously in apposition, for the *cavity* of the uterus, in the ordinary condition of the organ, exists *in posse*, rather than *in esse*. The uterine walls will thus be strained and stretched, and contractions will follow to expel the accumulated blood. These contractions will of course be the more intense, that is, the pains will be the more violent, the narrower the canal of exit and the greater the resistance. If the mass of blood has become coagulated the resistance will be considerably increased. Scarcely any objections can be raised against this theory as to the nature of the mechanical dysmenorrhœas, those at least that have been made are based upon utterly incorrect premises. It has been urged that fluid blood escapes drop by drop out of a much narrower glass tube, particularly when it has accumulated to a certain extent above the tube, but this objection is of no significance as applied to the case before us, for the canal of the cervix is in no sense a tube with stiff walls, and the slightest accumulation of blood in the uterine cavity is sufficient to cause the symptoms which have been described. It is more surprising that in the typical cases of stenosis of the canal of the cervix we sometimes meet with accounts to the effect that the first appearance of menstruation was unattended by any particularly violent pain, but that month after month the attacks became worse and worse. The cause of this, however, is easily to be found in the conditions to which the cervical stenosis gives rise. When the menstrual process

begins, only a small quantity of blood is excreted, and this may easily escape through the narrow canal without exciting violent symptoms, but as soon as the mucous membrane becomes in the least degree more swollen than it was before, a few drops of blood may remain behind; this excites the mucous membrane of the uterus and its cervix, and readily induces more swelling; the congenital obstruction becomes much increased, and violent contractions are required to overcome it—the case becomes one of pronounced dysmenorrhœa. In these cases the assertion that the increased swelling of the mucous membrane is the cause of the dysmenorrhœa would only be an idle contention, the swelling is indeed only the consequence of the stenosis, and the serious effects which it produces are only due to the simultaneous existence of this latter condition. Besides this, the influence here indicated, which the stenosis exerts upon the mucous membrane of the cervix and uterus is of far greater consequence. Serious alterations of the uterus, the results of these monthly attacks of colic, cannot fail to occur, and are found in all cases on close examination.

In the first place, catarrhal swelling and hyperæmia of the mucous membrane will very soon set in, and show itself, even in the absence of menstruation, as cervical catarrh; the more this swelling increases, the greater, as a matter of course, will be the impediment to menstruation, and the more violent the uterine contractions for the expulsion of the menstrual blood. This explains the fact, demonstrable in so many cases, that the dysmenorrhœa always goes on increasing, especially if by marriage the patients are exposed to additional causes of excitement and congestion. The repeated contractions of the uterus very soon induce hypertrophy of its walls, and the accumulation of secretion, both blood and mucus, distends the cavity, and thus a condition is produced which I have designated as hypertrophy with dilatation, or excentric hypertrophy, of the uterus, analogous to similar changes in the heart. These anatomical changes sufficiently explain two facts; the first is that the symptoms of mechanical dysmenorrhœa come on gradually and then always increase, and the second, that after a certain time has elapsed, if the changes have existed for some time, no striking effect will be produced by removal of the impediment, inasmuch as the consecutive changes that have taken place in

the uterus now amount to an independent affection, and never again subside. I will, in passing, just remark that sterility is always present in these cases of stenosis, and, according to my experience, it is only in extremely rare cases that simple dilatation of the canal of the cervix is sufficient to cure this condition, the consequences above described having a great deal to do with the subsequent course of events. Speaking generally, I scarcely know a more typical form of disease than that of mechanical dysmenorrhœa. It frequently happens that individuals suffering from this affection determine to seek the advice of a physician only when, after marriage, the ever increasing troubles become unbearable, and their sterility makes them anxious. In all these cases, the account given is, that the dysmenorrhœa existed before the marriage, and that it has gradually become worse and worse since its commencement. After what has been above described, all this is easily intelligible. In cases of stenosis of the external orifice and narrowing of the canal of the cervix without dysmenorrhœa, I have always been able to demonstrate defective development of the uterus generally, and these are just the cases which are often enough brought forward to show that stenosis of the cervical canal may exist without dysmenorrhœa. It is impossible to understand how anything can be adduced from cases such as these against the correctness of the views just defended, for the two conditions are entirely different.

There is at the present day no doubt whatever that dilatation of the cervical canal is the only means of remedying the above described form of dysmenorrhœa, and there are sufficiently abundant cases to prove that by this method of treatment the patients are often at once relieved of all their sufferings. The method by incision is the only one which ensures a tolerably complete dilatation, all other methods involving the use of absorbent substances are only transient in their effects. It is, however, certainly sometimes necessary to make use of absorbent tents in order to dilate the canal so that cutting instruments may be introduced. Whichever method of making the incisions may be adopted appears to me, generally speaking, to be a matter of no consequence, but those double knives ought unconditionally to be rejected, which make the freest incisions in the parts adjoining the internal orifice, and very limited ones in the external orifice of the uterus (Simpson's Hysterotome).

There is the less reason for my entering into a discussion of special methods, and details of the important after treatment, as these subjects have been completely, as well as admirably, handled by Olshausen in this collection of clinical essays. It is easy, according to my experience, to avoid the undoubted dangers attendant upon incisions of the cervical canal, by cutting the internal uterine orifice as little as possible, or indeed not at all, and by conducting the after treatment with the greatest care, above all things enjoining on the patient absolute rest in bed for some days. The division of the internal orifice of the uterus is, as I have repeatedly convinced myself, certainly not always necessary for the success of the operation, for the limited stenosis of this part occasions but a very small amount of obstruction as compared with the stenosis of the entire canal of the cervix.

We cannot deny that after any of the various methods of dividing the cervical canal, it is often difficult to prevent the parts from again becoming closed—in this respect, all the methods which have been adopted resemble each other. For some years I have always made use of Greenhalgh's Metrotome, and I consider it to be the most convenient and best instrument for this purpose. The cutting edge of this instrument is excessively sharp, and the incisions made by it are very prone to heal, but I find that this drawback may always be obviated by dividing the uterine orifice not only from side to side but by making a crucial incision. To do this I re-insert the instrument and cut in the direction of the antero-posterior diameter of the pelvis; the wound thus caused has but little tendency to unite. In this respect, however, it is important to attend to the after treatment, which consists in the insertion into the canal of the cervix, of tents soaked in liquor ferri, and these must be persevered with for a long time. If the patients are seen again some time after the operation has been performed, the much diminished size of the previously wide gaping orifice will usually cause some surprise; but I have never met with a case in which the cervical canal has become as narrow as before the operation, or in which dysmenorrhœa has recurred. It is only in rare cases that I have not observed the most striking improvement or even complete relief of all the menstrual troubles to result from this operation. In a small number of unsuccessful cases, the fault was in

the performance of the operation; just as the incision is made the patients sometimes give a sudden start, so that the metrotome slips out of the cervical canal and makes but a very small incision. The external orifice may be thus sufficiently widened as to allow the operator to think that it is dilated to the requisite extent, and unless this error is immediately perceived, there will, as a matter of course, be no good result when the next menstruation takes place. If, however, this failure is once brought to notice, its occurrence will immediately be recognised on attempting after the incision to insert the pledget into the cervical canal, and under those circumstances the little operation should be immediately repeated.

There is yet a larger series of unsuccessful cases, the reason for this being that they were unsuitable for the operation. My early experience certainly was to this effect. These are those cases of defective formation of the entire uterus which have been already described, and in these, if the ovaries are also in a similar condition there is usually an absence of dysmenorrhœa, but it sometimes happens that the ovaries are normally developed. These, then, are cases of ovarian dysmenorrhœa, in which, as a matter of course, no good result can possibly follow the division of the imperfectly developed cervical canal. Mistakes of this kind can only be prevented by careful investigation of the size of the uterus.

It is true that in some few cases the consequences of the stenosis, which have been described, the hypertrophy of the uterus, become so considerable that menstruation will still be troublesome even after division of the cervical canal, but these troubles will usually be less violent than before and will often gradually subside.

If I now, in conclusion, allude only briefly to that disorder of menstruation which has the greatest claim to be regarded as an independent affection—I refer to dysmenorrhœa membranacea—the reason for this is that I have nothing new to offer from my own experience. Many of the cases classed under this head are those in which abortion has taken place, but no kind of doubt can be entertained that the expulsion of the decidua menstrualis *in toto*, or as large shreds, may occur as a morbid process at each menstrual period in cases where the idea of conception or even of cohabitation must be excluded. After what has

been explained with reference to the nature of menstruation, the dissolution of the decidua, it is certainly intelligible that continuous layers of the decidua menstrualis should be expelled as membranous shreds when this breaking up occurs in consequence of hæmorrhage into the deeper layers of the mucous membrane (Solowieff) or other disorder, and not in the usual way. We can at any rate easily understand that according to the depth at which the separation of the mucous membrane *in toto* ensues, it will happen that sometimes there is an absence of glands in the extruded membrane (R. Maier and Hegar), sometimes these are all present (Hausmann); and sometimes they are only to be found in separate portions. (Saviotti observed the cæcal extremities of the glands to be wanting.) In like manner, the observation that in one menstrual period the decidua may be twice expelled as a membranous sac corresponding to the form of the uterus, as I myself have seen, finds a ready explanation in the fact that at first the superficial layers of the decidua, and then afterwards the deeper layers, are detached as a connected membrane. This has also been described by Solowieff.

With regard to the therapeutics of this affection, I have hitherto adopted the usual course, and applied various caustics to the uterine mucous membrane after previous dilatation of the internal orifice, but I cannot say that any permanent good effect has been produced.

ON THE

# CLINICAL RESULTS OF THE LISTER TREATMENT OF WOUNDS,

AND ON THE

# SUBSTITUTION OF SALICYLIC ACID FOR CARBOLIC ACID.

BY

PROF. C. THIERSCH.

THE treatment of wounds is now more frequently discussed amongst surgeons than formerly. Which do you consider the best method? would you yourself wish, in the case of a compound fracture, to be treated by the open method, or antiseptically, or do you still adhere to the usual closed dressing? are questions not unfrequently heard.

The lesions here spoken of are such as threaten danger, not by the implication of vital organs, but by the accession of incidental wound-diseases. In the first rank are to be named wounds in the greater operations, such as amputation-wounds and compound fractures, and the share which incidental wound-diseases have in the tables of mortality is well known in hospitals.

That the air in which the patient lives plays an important part is a surmise and indeed a conviction entertained long before it was proved that atmospheric ferments exist. Lister regards the action of these ferments upon the wound as the preliminary condition of incidental wound-diseases, and believes that if we can prevent this action during the healing of the wound, that process will be rendered aseptic, and all danger of zymosis thus avoided.

While formerly inflammation in wounds, in so far as it may be regarded as the consequence of the mechanical injury, was taken as the main point for consideration, and the therapeutic problem sought to be solved by antiphlogistic treatment, the chief weight is now laid upon the septic processes, all dangerous increments of inflammation in the wounds are attributed to them, and the *apparatus antiphlogisticus* is regarded as inefficacious against these processes.

Vegetable micro-organisms, bacteria, contained in the dust of the atmosphere, are especially regarded as causes of decomposition and poisonous agents.

Unfortunately many problems in this field still remain unsolved. Are bacteria the only causes of decomposition, are there different kinds of bacteria or only different stages of development of one and the same kind, are all bacteria deleterious, or are some harmless, do they act deleteriously in themselves, mechanically, or as poisonous matter, or is their action only a mediate one, in that they separate poisonous matters from the substratum upon which they act, can they decompose directly the fluids of the healthy body, or do they require a soil prepared for them by pathological processes, even the cardinal question what decomposition is, how the separative processes which are proper to it differ from those effected by physiological changes of matter? appears to me still unexplained. The views of special inquirers differ but too often, and—"who shall decide when doctors disagree?"

It is, at all events, ascertained that the atmospheric ferments exert a special action upon decomposable fluids, to which the secretions of wounds belong, as the consequence of which we may regard decomposition in the stricter sense of the word; further, that the secretions of wounds, in which I include the parenchymatous fluids of the wounded parts, react variously upon the organism in accordance with their own composition. All the rest is a question of probability. I agree with those who believe that the action of the atmospheric ferments furnishes the preliminary condition for sepsis, hospital gangrene, and pyæmia, I assume that by chemical decomposition, under the influence of the ferments, poisonous matters are set free, and that the ferments are not in themselves poisonous. The rest I leave an open question.

Here also, as so often elsewhere, the physician cannot wait until the precious metal is separated from the ore, and circulates with the stamp of scientific coin. The requirements of the day compel him, even if the scientific hypothesis is incomplete, to make the therapeutic experiment, in which, however, he must not forget the *non nocere*. I regard Lister's treatment of wounds as a therapeutic experiment of this kind which has proved successful.

In the first part of this work I have made the attempt, by means of a critico-historical glance backwards, to connect the antiseptic treatment with what was done in earlier times. With the pleasing interest now taken in the history of surgery, this attempt requires no apology, but I have not been so successful therein as I could have wished; in spite of the previous works of Billroth, Trendelenburg, Krönlein, and others, the necessary leisure could not be found. I feel the deficiencies, and it is reserved for a more experienced pen to do justice to this interesting chapter in the history of our development.

The second part treats of the first cases in which I employed the salicylic acid, recommended to me by Prof. Kolbe, instead of carbolic acid, in the Lister treatment of wounds.

The third part contains all the cases treated antiseptically in the Leipzig Hospital during the last ten months, summarily arranged with critical remarks.

The last part consists of some communications concerning the production and testing of the preparations of salicylic acid used in the Lister treatment of wounds, and of short directions for their employment, based upon the results at present attained.

## I.

When, in the year 1536, a strong army of the "great King Francis" entered Piedmont by the Mont Cenis, it was accompanied by an inexperienced surgeon nineteen years old—still very "doux de sel," as he himself says—the afterwards so celebrated Ambroise Paré. The storming of the small mountain fortress Villane, near Susa, probably gave him for the first time full occupation, and he followed in all things the example of his older colleagues. Like these, although hesi-

tatingly, he poured into the gun-shot wounds boiling oil of elder to destroy the poison, but the oil fell short, and thus he was compelled to dress the other wounded men with an ointment of oil of roses and turpentine. Fearing that the latter would soon become victims of the wound-poison, he passed a sleepless night, got up early to see the ill consequences, but was greatly surprised to find those he had half given up free from pain, and without inflammation or swelling, while those who had been treated with the boiling oil lay in a state of fever, with great pain and much swelling. He therefore determined, as he tells us, never again to burn the poor subjects of gun-shot wounds so cruelly.

Paré's treatise on gun-shot wounds, in which he asserts the non-poisonous nature of them, and in which the above account is included, first appeared in Paris* in 1551, fifteen years later. A part of this interval he passed in Italy, and it is supposed that he became acquainted there with the investigations of the learned doctor of Bologna, Bartholomew Maggi, and adopted his line of argument against the poisonous nature of gun-shot wounds. Maggi's treatise appeared a year later at Bologna.†

However the question of priority may stand, it is certain that it required all the respect which Paré enjoyed in riper years, especially in regard to military surgery, to obtain gradually consideration for the new view. In much later years we still encounter the old error.

How this error could gain such strength and extension can be proved with difficulty, however, in single instances. It is certain that gun-shot wounds were at first regarded as contused wounds, and were therefore treated, according to custom, with warm and moist applications. But since gun-shot wounds from their very nature, especially when complicated by fractures, are disposed to take a bad course, and since this bad course must have become more frequent as the number of such lesions has become greater since the introduction of small-arms, while the precautions formerly taken for the welfare of the wounded were insufficient, experience at the bed-side could but lead to

* Manière de traiter les playes d'Arquebusades et flêches. Paris, 1551.

† De vulnerum bombardarum et sclopetorum globulis illatorum et de eorum symptomatum curatione tractatus. Bonon., 1552, 4.

and confirm a very unfavourable view of the especially dangerous nature of such wounds. Cases of acute sepsis, developing itself in the course of the wound and agreeing in essential points with the consequences of the bite of a poisonous snake, will have given rise to the idea. Even during the late war, cases of the kind occurred which now and then excited a suspicion in the minds not of laymen alone that the enemy made use of poisoned missiles.

Thus in the beginning of the sixteenth century Johannes de Vigo had only to put into a dogmatic form the already established view of the physicians of the time. A gun-shot wound is a contused wound, he says, for the bullet is round; it is burnt, for the bullet is heated; it is poisoned, for powder is poisonous. The poisoning is the essential condition, therefore the treatment must be directed above all to counteract this. And thus, in consequence of the great reputation of J. de Vigo, the fate of sufferers from gun-shot wounds was determined for many years. Amongst various escharotic methods employed against the poisoning, the pouring of boiling oil into the fresh wounds was that which recurred most frequently.

All this is rather far-fetched, it will be said, when we are speaking of the present state of things in reference to the treatment of wounds, but the distance of three centuries and a half is only apparently a great one. The theoretical error is no doubt removed, but the fact which it sought to explain is not removed with it. We no longer look for the poison in powder and lead, but we speak, as they did formerly, of a poisonous action of the wound upon the body: this poisonous condition forms now as then the main point of our therapeutic endeavours, in fact, J. Lister's first method of cauterising the fresh wound with concentrated carbolic acid has no doubt reminded many of the burning out of gun-shot wounds in the fortress of Villane.

The next step was that a poisonous substance may develop itself or settle in the wound, and especially in gun-shot wounds, a substance which has nothing to do with powder or lead. Paré himself adopted this view. When he took part in the siege of Rouen, many wounds sloughed and had a cadaverous smell, and on opening the bodies of those who died, numerous collections of pus were found in different parts of the body,

full of greenish ill-smelling ichor. Besiegers and besieged believed themselves to be wounded with poisoned bullets. Paré looked for the cause in a deterioration of the air by the large quantity of decomposing substances, and since he admitted of a caustic treatment of the wounds in such cases, cauterisation with unguentum ægyptianum (equal parts of alum, verdigris, and sulphate of copper), he appears to have assumed, as is done at this day, a direct action of the so-called deteriorated air upon the wound itself.

The evil influence of air vitiated by the products of decomposition, not upon wounds only, but upon the organism generally, has never been lost sight of by physicians since that time. That rotten straw, decomposing bodies of men and animals, surfaces saturated with excrement, and overcrowding of badly ventilated hospitals give rise to infectious fevers and an unhealthy state of wounds, is not a result of modern observation only. That it was a question of processes of fermentation which became communicated to the body by means of the excitors of fermentation contained in the air, was a view frequently adopted. To quote one only out of many: John Pringle, in his "Observations on the Diseases of the Army," published in 1775, devotes a chapter especially to 'Diseases resulting from Bad Air,' and his forty-eight experiments on septic and antiseptic substances contain numerous hints at attempts resembling those made at the present day to determine the antiseptic power of certain things.

No advance was made, however, beyond vague surmises concerning the nature of the excitors of putrefaction, and they were for the most part looked for amongst the volatile, ill-smelling products of decomposition, and were believed to be extremely subtle gaseous matters. It is now known, and this knowledge has become public property through the modern literature on fermentation and putrefaction, that it was reserved for the investigations of the present day to obtain possession of these substances, and I can therefore treat this part of the subject briefly. These investigations were directed first to the *generatio equivoca*. That in a fluid capable of putrefaction, which had been exposed a considerable time to a boiling temperature and brought into contact with air previously heated or passed through sulphuric acid, not even the minutest organisms

appear, was first shown by Fr. Schulze*, Schwann†, Helmholtz‡, and further that under these conditions putrefaction does not take place. If the organic character of the excitors of putrefaction was proved hereby, they might still be regarded as possibly gaseous substances. The decisive experiment of Schröder and Dusch§ put an end to this doubt, since it showed that it was indifferent for the success of the experiment whether the air was heated, submitted to the action of sulphuric acid, or passed through cotton. Since cotton can have no other effect upon the air except that of retaining the matters contained as dust in it, the excitors of putrefaction were to be sought for in these. In connection herewith stand the experiments of Pasteur,‖ which now, amongst German authors also, are the only ones usually mentioned.

The matters removed from the atmosphere by the cotton in Schröder's experiment, or by the bent tubes in that of Pasteur, consist partly of detritus of all kinds, partly of extremely minute organisms, and as regards their production it appears to me to be proved that the micro-organisms which make their appearance in boiled infusions come from the air, and it also appears to be proved that these micro-organisms exert a decisive influence upon the decomposition of their infusions. It is quite otherwise with the assertion frequently heard that decomposition can be caused and carried out to its final products by micro-organisms only; here at once the doubt presents itself whether the atmospheric ferment-dust, seeing that the micro-organisms can only reach the air through the drying up of their infusions, does not also contain fragments of this dried fluid, and whether these fragments are not also capable of setting up a decomposition. We are acquainted with physiological ferments which, even if arising in cells, are soluble in water,

* Preliminary communication in Poggend. Annal., B. 29, p. 487.

† Preliminary communication concerning experiments on wine fermentation and decomposition. Poggend. Annal., 1837, B. 41, p. 184.

‡ On the nature of decomposition and fermentation. Müll. Archiv, 1843, p. 453.

§ Schröder and Dusch, On filtration of the air in connection with putrefaction and fermentation. Annal. der Chem. und Pharm. 1854, Bd. 89, p. 232. Schröder, *loc. citat.* 1859, Bd. 109, p. 35. 1861, Bd. 117, p. 273.

‖ Mémoire sur les corpuscles organisés qui existent dans l'atmosphère.

and when dried retain their ferment properties, and why should such soluble ferments not also exist in putrefaction-fluids? So long as we do not succeed in separating the micro-organisms completely from their infusions, or in isolating them from the atmospheric dust, or so long as we possess no method of destroying the micro-organisms in a fluid capable of putrefaction, without modifying the albuminates, we must leave this an open question.

Sufficient for the purpose of this treatise is the fact that the atmosphere always contains putrefaction-ferments in the form of dust, and that, therefore, when we speak of foul air and pure air, this only means a difference in the quantity, perhaps also in the quality of these ferments.

So far as I know, Lister was the first to refer the long-known essential difference in the behaviour of open and subcutaneous wounds to this fact, while one had previously been content to assume that the difference between subcutaneous and open wounds depended upon the access or non-access of oxygen to the fluids of the wound. It being once established that the atmosphere always and everywhere contains ferments, it was evident that the secretions of wounds contained just such combinations as are especially liable to be changed by ferments. In this sense, every wound exposed to the air must become infected or poisoned, and to obviate this poisoning and effect an aseptic course must be accepted as the chief object in the treatment of wounds.

Lister argued that if we succeed in destroying the ferments which have entered into the open wound before they come into activity and during the healing of the wound admit only disinfected air, *i.e.* air in which the ferments have been destroyed, the wound will run an aseptic course like a subcutaneous wound.

One of these questions concerns the behaviour of the atmospheric ferments contained in the healthy organism, for on account of their minuteness and their distribution in air and water, they must be regarded as always present in the organism. Do they undergo destruction there, or are they merely latent? Billroth* inclines, on the ground of his investigations, to the

* Investigations of the vegetation-forms of coccobacteria septica, &c. Vienna, 1874.—Billroth's surmise that these latent resting spores, so soon

belief in their latent presence in the healthy body, and the experiments of Tiegel* serve to confirm this view. But if they are merely latent, they will be brought by the circulation from behind to the wound, will find in the secretions of the wound, air having access, matter fitted for their activity, and little will be gained by the prevention of their free access from the atmosphere.

The second question is, how do the unformed ferments coming from the organism, which are contained in all the albuminous fluids of the body, including the secretions of wounds, comport themselves when brought into contact with air which has been deprived of its ferments? Certainly otherwise than in subcutaneous wounds to which access of the oxygen of the atmosphere has also been prevented. In the Lister treatment also a continued antiseptic influence upon the secretions of the wound is kept up, and no one can doubt that the constant presence of carbolic acid in the neighbourhood of the wound, which is one of the essential characteristics of the Lister treatment, must have an important influence on the chemistry of the secretions of the wound. In the Lister treatment the oxygen of the atmosphere has access to the wound and the secretions of the wound are under the action of carbolic acid: the wound, therefore, is not placed under the conditions of a subcutaneous wound.

Another question is, whether Lister has succeeded in rendering the healing process aseptic without otherwise exposing the patient to danger. Lister is constantly endeavouring to improve his method and render it milder. Carbolic acid continues to be his most important means of disinfection. Instead of the original cauterisation of the surface with concentrated carbolic acid, he washes it with diluted carbolic acid, or with a solution of chloride of zinc in the proportion of 1 : 8. Carbolic acid spray is employed to destroy ferments entering into the wound during the operation, as well as when the dressing is renewed. The tying of the arteries with carbolised catgut was an important addition, and instead of the paste-

as some pathological process furnishes them with a fitting soil, may at once start into activity, appears to me to be of great importance.

* On coccobacteria septica (Billroth) in the bodies of healthy vertebrate animals, by Dr. E. Tiegel. Virchow's Arch., 1874, Nos. 3 and 4, p. 453.

dressing, that with carbolised gauze is used, which permits the easy escape of the secretions. This escape is further facilitated, when it appears desirable, by the introduction of carbolised india-rubber tubes. Thus originated the Lister dressing, to which the objection that it endangers the patient can no longer be made, and which is now strictly carried out in many places, *e.gr.* in the hospitals of Halle, Basle, and Leipzig, and the steps of which are accurately described in the treatises of Dr. A. W. Schultze* and Dr. L. Lesser,† which I assume to be already known to the reader.

The results of the Lister method at the bed-side were certainly of a nature to attract at once the attention of all those who, not content with their previous success, wished to prevent the deaths resulting from pyæmia, septicæmia, and hospital gangrene. Lister's‡ communications made an impression of reality, and all that one heard of him from those who went to see him at Glasgow, and afterwards in Edinburgh, sounded favourably. I was the more inclined to employ the Lister method from the first, because the Leipzig Hospital at that time (1867) was still in the old Jacob's Institution, one of the worst of hospitals, and I confess that I was one of those who, in accordance with the original instruction, cauterised the cavity of the wound in cases of compound fracture with concentrated carbolic acid. It unfortunately happened sometimes, that without effecting an aseptic course in all cases, slough-ing of tissues of serious extent occurred. With the present method there is no fear of this. Our results have constantly improved with the development of the method and with our technical experience. They are not so good, it is true, as those of Lister himself, or of Volkmann, but I feel justified in recommending the method, and am of opinion that it ought to be tried impartially, more than has yet been done, if only

* R. Volkmann's Collection of Clinical Lectures. No. 52. 1872.

† Deutsche Zeitschr. für Chirurgie, by Hüter and Lüke, 1873, p. 402.

‡ Those of Lister's publications to which I have as yet had direct access were contained in the *Lancet* in 1867—March 16, 23, 30, April 27, July 27, and in a paper in the *Microscopical Journal* for October, 1873. The rest I know, in so far as they have been made use of by Schultze and Lesser. The work by Dr. A. Thamhayn, "The Lister Dressing," Leipzig, 1875, contains all Lister's articles, but, unfortunately, did not reach me in time. I trust, however, that I have not missed anything essential.

because the methods of treating wounds hitherto leave much to be desired.

An attempt to give a comprehensive description of the modes of dressing which were in use, or are so still, presents much difficulty on account of the variety and quantity of the material. The best way appears to be to obtain a general view by determining how each of these modes answered to the chief requirements of a practical treatment of wounds. As such chief requirements may be regarded: rest of the wounded parts from the first moment, unimpeded escape of the secretions of the wound, prevention or arrest of septic processes. For simplification I will confine myself to large amputation-wounds and compound fractures, partly because these most frequently lead to those evil results which we seek to prevent, and partly because what holds good for them holds good for less severe wounds also. Of injuries of the cavities and their organs, I shall not speak here. Neither shall I allude to the influence of warmth and cold beyond observing that the latter will be chiefly of use where it is a question of the immediate mechanical change occasioned by the injury alone, not of septic complications. As an antiseptic it is of little value.

The open treatment of wounds, as being the most simple, must here be spoken of first. When it was first carried out methodically, and recommended to others by the Vienna surgeon Vincenz von Kern in the beginning of this century, it appeared to the majority of surgeons to be sheer heresy, and although Ph. v. Walther warmly recommended it, and Jüngken reported favourably of what he had seen in Kern's practice, it was not admitted to the larger hospitals, so strong was the prejudice that a wound must be defended from air and required certain local medical treatment for its cure. The great ascendancy of French surgery at that time, which adhered to ancient traditions, was no doubt the chief impediment.

Stromeyer returned to the principles of Kern, and I saw under his management, during the Danish war of 1850, many gun-shot wounds with fracture and amputation-wounds treated successfully by the open method, and also during the last war I saw a patient with mortified leg, the spontaneous separation of which under the open treatment Stromeyer awaited, and, as I afterwards heard, with good success. It naturally requires

the soft, tender, and patient hand of a Stromeyer for the success of such an attempt, and the never-to-be-forgotten example which he gave us younger men in 1850, was witnessed once more by me twenty years later. In the fifth and sixth decade of this century, Vezin, Bartscher, and A. Burow also adopted this method. In July, 1866, I saw in the Assembly Room of Kissingen stumps treated by Burow alongside of others, and so far as I can remember, all suffered equally under the unfavourable conditions present. I have myself made many attempts therewith, and so, doubtless, have many others, but the real value of this method was first established statistically by the report of Dr. R. U. Kroenlein* on the results attained by the strict carrying out of this method by Rose in the Zürich Hospital, as compared with the close dressing formerly in use.

Whether the wound lies quite free, or is covered with a piece of oiled silk, or with a water compress cold or warm, does not appear of importance, if free escape of the secretions is not prevented thereby; we may also add irrigation without changing its character, and the permanent water-bath may also be of use.

It affords the wounded parts perfect rest and obviates all retention of the secretions, meets therefore two of the chief requirements completely, and also antiseptically is not without effect; for while the wound is placed by the rest in a condition to close itself towards the exterior by sealing up of the cavities of the cellular tissue, and any products of decomposition which may be formed in the wound are not retained, so can also retained, decomposed blood or secretions not be pressed into the still unclosed or re-opened cavities of the cellular tissue, which is certainly the most frequent starting-point for septic and pyæmic processes. I have repeatedly seen large wound-surfaces, *e.g.* those of amputations of the thigh with extensive sloughing eschars, proceed to cleansing and granulation without a trace of fever, which is certainly the best proof how completely the putrid fluids run off externally. The superficial mortification which even the cleanest incised wound must undergo when exposed to the air need not be taken into con-

* The open treatment of wounds from experiences in the surgical wards at Zürich, 4, 1872.

sideration, since healing by the first intention was never expected.

In opposition to the great advantages of the open treatment of wounds stand as disadvantages,—that it permits the formation of products of decomposition on the surface of the wound, which may sometimes find their way into the circulation before the granulation of the surface of the wound takes place, or through a wound in the granulating surface; that it presents an undefended surface for the deposit of the infectious matter of hospital gangrene; and that it also, in cases otherwise favourable therefor, partly or entirely gives up any attempt to effect the immediate healing of the wound.

To obviate the first of these disadvantages, Bardeleben's method combines the open treatment with antiseptic irrigation of the surface of the wound.

Since the leaving open of the wound does not always in itself alone permit the free escape of the secretions, especially in cases of compound fracture, and since such free escape is one of the most important points in this treatment, the various precautions and measures which facilitate escape serve for its completion: dilatations by incision, opening of fluctuating points, counter-openings previous to, or after deposits of pus; drainage in the place of the dossils of lint and setons formerly in use, etc.; also the free opening of wounded joints and resection of them, may be included here.

A contrast to the open treatment of wounds is presented in the attempts to place the wounds under the shelter of a firm eschar dried *in situ*, whereby the access of air is prevented and the condition of a subcutaneous wound approached. Exclusively of slighter and superficial lesions, this method has furnished good results in the case of compound fractures of a slighter kind with small skin wounds, and a remarkable instance how, under the defence of a closely adherent eschar consisting of wadding, blood, and chloride of iron, sprinkled with carbolic acid, denuded cranial bones became converted into granular tissue without appreciable suppuration, is given in Trendelenburg's treatise, "Cure of injuries to bones and joints under an eschar."* This method served Lister as the starting-point for his first method, and indeed, if the formation of the eschar is

* Archiv, für klin. Chirurgie, 1873, vol. xv. p. 461.

preceded by the disinfection of the wound and the eschar prevents access of the atmospheric ferments, the preliminary conditions of an aseptic course are given. In the usual method, however, recommended by A. Cooper, of letting the eschar of blood and lint dry without previous disturbance of the wound, or of forming the eschar by the addition of glutinous or coagulating substances, suppuration will take place in severe and deep contused wounds generally on the third or fourth day at the bottom of them, with swelling, pain, and fever, and it will be fortunate if this disturbance pass away with the removal of the retained pus. The method has this advantage, that it insures perfect rest for the wound during the first few days, and may, under some circumstances, effect an aseptic course, but since, in cases of deep-seated suppuration, it checks the escape of the pus, it may cause grave complications.

It has not been used, so far as I know, in large amputation-wounds, but we may mention here the treatment of amputation-wounds with the écraseur and the red-hot wire, and the cauterisation of the fresh surfaces of amputation-wounds with wadding steeped in chloride of iron, as was practised occasionally by French army surgeons during the last war. This was done under the notion that with the defence of an eschar produced by compression, actual cautery, or cauterisation, the surface of the wound becomes closed more readily externally, before decomposition takes place, than in an incised wound left entirely to itself. But as there is no probability that these interesting experiments will be adopted generally in practice, I content myself with remarking that wounds so treated are exposed to the same dangers as other such wounds healing slowly by granulation.

We may also name here Jules Guérin's hermetically closed india-rubber dressing with suction apparatus. In 1765 the Florentine, Vacca, published a work, "Liber de inflammationis morbosæ, quæ in humano corpore fit, natura, causis, et curatione," and Vacca, who knew that decomposition is only possible when air has access, spoke in detail of various antiseptic measures, and ended with the proposal to cover the amputation-stump with lint and oiled linen, to place over this a leather cap, and then to enclose the whole in a glass bell with air-tight rim. The air enclosed would do no harm because in small quantity. To

pump it out was not advisable, because the stump ought to remain under normal atmospheric pressure. This proposal has probably never been carried out, but it shows how long the idea of the deleterious effect of air has occupied the attention of surgeons.

Here belongs also Alph. Guérin's dressing with wadding, in which, by covering the wound with a thick layer of wadding to be left on for a considerable time, the infectious matters of the air are excluded, and since Ollier sprinkles the wadding used in this dressing and the wound itself with carbolic acid, this is a close dressing which may act antiseptically, not only by excluding atmospheric ferments, but also by destroying any ferments contained in the wadding or in the wound.

What has been said of treatment under an eschar holds good also for these close dressings. The rest which the wounded part enjoys for some time is certainly favourable, but the antiseptic action is not much to be relied on, and if sufficient care be not taken to allow of the escape of the secretions, evil results from their retention are to be feared.

The dressings for amputation-wounds and compound fractures which are probably now most widely used, consist, as is known, in covering the wound with an absorbent porous substance, formerly exclusively lint, now frequently wound-wadding, either laid immediately upon the wound, or with interposition of a mild salve. This dressing was to be regarded as a step in advance, as it took the place of continuous irritation of the wound-surface by strong ointments, corrosive solutions, and liniments. But when there is much discharge, it requires to be changed frequently, which interferes with the rest for the wound, and it not unfrequently causes retention of pus, and facilitates decomposition of the secretions of the wound. It therefore decidedly does not meet the three chief requirements: rest, free escape of secretions, and antisepsis, and unless I am mistaken, has already passed the maximum of its extension. In amputation-wounds, in which healing by the first intention is aimed at, it so frequently leads to a superficial healing of the edges of the skin, while septic matters are developed deeper in, that the healing of the wound by the first intention, although recommended by the greatest surgeons of England and Germany, has never been generally adopted; it

was thought better to abstain from the attempt than to risk failures so often accompanied by evil consequences.

Information concerning the at present prevailing views of French hospital surgeons is furnished by a discussion which took place on March 25, 1873, at a sitting of the "Association française pour l'avancement des sciences," at Lyons. The occasion for this was a communication from Dr. Azam of Bordeaux, in which he most warmly recommended the exact union of amputation-wounds. In his method the wound is carefully closed by a deep-seated and a superficial row of sutures, while the retention of the secretions is provided against by drainage. Over this is placed a dressing of wadding. Water and the sponge are not used at all. The sutures are removed on the third and fourth day, the drainage-tube on the tenth. The dressing is moistened with alcohol. Complete healing of stumps of the leg on the eleventh and thirteenth day, of the thigh on the fifteenth and sixteenth, are reported in recommendation of the method. Verneuil, without meeting with any opposition, formally declined all such experiments for the large hospitals, "autant de nécropoles assermentées," for which the slow healing of amputation-wounds is exclusively adapted.* In reference hereto, Alphonse Guérin, at a meeting of the Surgical Society, December 26, 1872, showed a stump (model ?) the flaps of which had united under his wadding dressing in fourteen days. One can therefore, he says, effect the healing of such wounds by the first intention in the hospitals of Paris. Guérin was unable to show the patient himself, who had died of embolic pyæmia resulting from suppuration of the spinal cord. It was, therefore, only a fallacious, superficial healing which was boasted of at that meeting as something particular.†

Those who have not adopted the open treatment of wounds are in the habit of filling out amputation-wounds with a pad of lint or a piece of sponge, which comes away after five or six days with a bad smell. If the patient passes this point, he has a prospect of becoming cured by the process of granulation.

The method whose place was taken by this indifferent close dressing and which determined practice for centuries, may be described as laying the chief stress upon a therapeutic treat-

* Gaz. hebdom., 1873, p. 575. † Gaz. hebdom., 1873, p. 13.

ment of the surface of the wound, and concurrently upon the prevention of access of air by means of close dressings, and is, therefore, now not unfrequently described as an irritant treatment of wounds, and in fact it has the fault of keeping up an inflammatory irritation of the wound, but essentially it is an antiseptic treatment. It was developed from the observation that every wound which does not heal by the first intention passes through a stage of cleansing, and that, during this stage, the wound is covered with decomposed matter. To effect a quicker removal of the dying tissues and a more rapid digestion of them, and to deprive the decomposed matters of their deleterious qualities, digestive applications were employed, chiefly in the form of ointments. Of the means used we can scarcely find one which is not possessed of antiseptic qualities, from the wine, vinegar, sulphate of copper, and verdigris of the older to the cinchona preparations of the later surgeons. The wound having become cleansed, the further treatment depended upon the state of the granulations, which were partly encouraged in their growth by mollifying, flesh-forming applications, partly checked by such as tended to dry them up. Side by side with the milder antiseptic treatment there was a more energetic one by which a corrosive eschar was aimed at, and I have already mentioned that Ambroise Paré, after he had given up the immediate cauterisation of gun-shot wounds, still admitted that there were cases in the later stages of which it was necessary to destroy the sloughing or ichorous surface of the wound by Egyptian ointment. If this treatment was accompanied by constant mechanical disturbance, such as violent syringing, the introduction of dossils of lint or setons containing acrid substances, it must certainly have done much harm, while its cautious employment is perhaps, at present, thought too unfavourably of. The name of Bilguer has a good sound, because he is justly regarded as the founder of the conservative direction in military surgery. If we examine into the means which he used to heal gun-shot fractures, in which at that time amputation was considered absolutely necessary, we see that his method, apart naturally from the very important operative steps, which are to be regarded as preparatory to later resections, and apart from the most gentle possible manual treatment, consisted in the application of very various antiseptic

substances. After the cautious removal of any sloughing tissues, the wound was most carefully smeared once or even twice a day in all its inequalities with antiseptic substances—mastic, turpentine, oil of cloves, camphor, alcohol, vinegar and wine, sal-ammoniac, saltpetre, caustic lime, Egyptian ointment, and many other substances being used by him. As a remainder of this we meet here and there with the custom of painting necrosed edges of bone with tincture of myrrh. In looking through his reports of cases we get an idea of the trouble and labour which each patient caused in this method; he certainly spared no pains in the conservation of limbs condemned to amputation by others. He states (p. 80) that of 6,618 wounded, 653 died, 408 were entirely or half invalided, while the remaining 5,557 were cured. "The 408 who were entirely or half invalided," he says, "must undoubtedly have had bad fractures, *i.e.* comminuted or contused wounds of bones, for it is well known to all that no soldier of ours can be invalided for wounds of the head or flesh wounds." Of these 408 invalided not one had been amputated; of those who died, many had wounds of the head, chest, or abdomen, while many succumbed to hospital diseases. These results appear to me to be good, and I think it right to give them prominence, even if they prove nothing else than that the otherwise so rational and careful treatment of Bilguer was compatible with this excess of antiseptic activity.

Neither the open treatment nor that with an eschar and exclusion of air, neither the simple close dressing nor that containing antiseptic substances, is quite satisfactory, and any one who is not prepared to put up with a certain percentage of deaths from pyæmia or sepsis as an inevitable evil will be ready to experiment with methods which combine good results with an appearance of rational foundation, even if he be distrustful of the theoretical premisses of the method and of extensive assurances. Such a method I believe that of Lister to be.

If we examine in how far it meets the three chief requirements of a good cure of wounds, rest, free escape of secretions, and antisepsis, we find that the latter is its strongest point. According to the assurance of its discoverer, it is capable of obviating any putrid decomposition in the district of the

wound or to check it at once, and that not, as in the first experiments, by cauterisation of the wound with concentrated carbolic acid, but by the constant action of very small quantities of that preparation. If we admit this effect of the dressing for the present and inquire how it stands with rest for the wound and with the free escape of the secretions, it must be confessed that rest is often interfered with by frequent change of dressing just in the first critical period, and that escape of the secretions is not so unimpeded as in the open treatment of wounds. The retention of blood and exudations, formerly so frequent, is, indeed, obviated to some extent by the india-rubber tubes, while the carbolised gauze, which is not sprinkled with water, lets the wound-fluids pass, as through a hair sieve, better than lint or wadding, but obstructions in the wound and between wound and dressing are, however, not wanting.

If antisepsis, or more correctly speaking asepsis, be effected, these faults are not of much importance, and will merely retard the cure, but if it fail, the patient is in a worse position than if the attempt had never been made; to the traumatic process has been added the irritating action of the carbolic acid, and in consequence of the retention, the entrance of septic matters into the circulation has been made possible, a circumstance which was spoken of in connection with the ordinary close dressing.

Although the prevention of sepsis appears as the main object of Lister's procedure, it is nevertheless applicable to cases in which sepsis has already occurred, but the danger is then naturally increased of harm being done by retention and irritation, for the probability of checking sepsis already established is but small. In compound fractures not treated at an early stage I therefore consider it dangerous to obstruct in any way the escape of the wound-secretions.

Also as regards the choice of the antiseptic, this method is not free from all objection. It has already been mentioned that Lister himself sought for means which, equally antiseptic with carbolic acid, are less irritating, for doubtless the probability of an antiseptic course falls if the dose of the antiseptic has to be diminished on account of the collateral irritative action. A solution of chloride of zinc in water 1 : 8, introduced into practice

by Morgan for washing out the wounds after extirpation of cancer, to free them from cancer germs, is recommended by Lister and also by Volkmann. Lister has also experimented with boracic acid, *e.gr.* dressing-wadding or lint steeped therein, but hitherto carbolic acid has been employed chiefly. Alcohol irritates still more than carbolic acid. With preparations of cinchona, apart from the expense, the antiseptic effect is not sufficiently powerful. The antiseptic substances which have been tried in this respect, either act reliably only when used of a strength to produce eschars, or they irritate too powerfully, such as camphorous substances, or they rapidly become inefficacious, when in contact with the wound, by chemical changes, such as combinations of chlorine, sulphuric acid and its salts, or alkaline hyper-manganates.

But not only is the inflammatory irritation which carbolic acid produces in larger quantities undesirable, but also the volatility of the carbolic acid. This makes it difficult to keep a sufficient store of antiseptic matter in the dressing for any length of time.

That, therefore, a substitute for carbolic acid is desirable for several reasons will be generally admitted, and this substitute is, I believe, furnished by salicylic acid.

## II.

In March, 1874, Professor Kolbe informed me that he had succeeded in producing the previously very expensive salicylic acid in a cheap way by synthesis from carbolic and carbonic acids, so that its employment on a large scale for commercial and therapeutic purposes may now be calculated on. In the latter respect, he hoped that salicylic acid might serve in a similar way to carbolic acid as a surgical antiseptic, and that without the unpleasant collateral effects of the carbolic acid. He sent me at the same time a considerable quantity of the acid recently prepared by his process. The acid is soluble at an ordinary temperature, in about 300 parts of distilled water, and this solution, which I shall call, for the sake of brevity, salicyle water, possesses considerable antiseptic properties. Urine to which salicylic acid has been added in this or in a larger proportion, does not decompose, though left exposed to the air. A

specimen still contains urea after nine months, and has not become at all turbid. In like manner behave blood and pus. Blood, on the addition of salicylic acid, undergoes precipitation and change of colour. It assumes a dirty violet colour, which probably results from the iron present in it. From the serum of pus salicyle water precipitates a larger quantity of albuminates than carbolic water containing one per cent. Fresh and granulating wounds undergo no inflammatory irritation when irrigated with salicyle water, and if, on continuing this for some time, a white layer of coagulum is formed on the granulations by precipitation from the serum of the pus, the granulation tissue remains uninjured beneath it. After irrigating wounded surfaces for a considerable time, salicylic acid appears in the urine, and the presence of the acid in the urine frequently betrays itself by an olive green coloration of the latter. Poisonous effects during the passage through the body, when given internally to the extent of 7½ grains or more in the course of the day, were not observed;* in a case of embolic pyæmia it produced no good effect.

The colourless blood-corpuscles and the pus-corpuscles retain their form, probably by the gradual induration of their protoplasm, but the red blood-corpuscles assume the appearance of yellow rings, with a bright centre, from accumulation of the colouring matter in the peripheric layer.

In the protoplasm of fresh pus-corpuscles and colourless blood-cells, the molecular movement of the granules embedded in the protoplasm continues for some time in spite of the presence of salicylic acid.

If we have to deal with the surface of a wound which is already occupied by the ill-smelling products of ichorous, or gangrenous decomposition, the action assumes a different form. After carefully cleaning the surface of the wound, sprinkling it with salicylic acid in substance suffices to prevent the recurrence of the bad smell, but if the ichorous or mortified parts are of a certain thickness, *e.gr.* from the twelfth of an inch to one-fifth of an inch thick, as in ichorous cancer-ulcers, frost or senile gangrene, the action does not penetrate deep enough, and the smell returns in a short time. Under some circumstances, the

* Professor Kolbe took gr. xxiii½. for several days in succession, by way of experiment, without inconvenience of any kind.

application causes burning pains, lasting from a quarter to half an hour.

It being thus proved that salicylic acid in the proportion of 1 : 300 prevents decomposition in blood and in the exudations from wounds, further that this solution causes no deleterious irritation on fresh and granulating wound-surfaces, and that no evil consequences are to be feared from the passage of the acid into the circulation, I set to work to employ it methodically in fresh wounds. As, in the Lister treatment, the access of atmospheric ferments to the wound during the operation was to be rendered harmless by carbolic acid, so this was to be effected here by salicylic acid, and at the same time a sufficient quantity of the antiseptic to be kept constantly in proximity with the secretions of the wound to act also upon other decomposition-processes in these secretions, independent of atmospheric ferments, by checking or modifying them. I employed for this purpose salicyle water and wound-wadding containing crystallised salicylic acid, which, for brevity's sake, I shall call salicylic wadding. I had this wadding prepared in the proportion of 3 and 10 per cent., and to prevent confusion had the latter coloured carmine. I left the salts of salicylic acid out of consideration, as they possess but little antiseptic power.

Since salicylic acid is not volatile at ordinary temperatures like carbolic acid, it at first appeared doubtful whether it would be equally efficacious in the form of spray for rendering the atmospheric ferments innocuous, but it has proved that the deposit which the spray forms upon the wound fulfils the antiseptic purpose, so that we may employ, with equal confidence, salicylic or carbolic spray. The salicylic spray is naturally inodorous, but provocative, until a certain toleration has been established, of sneezing and coughing, and the same effect is produced by the fine salicylic dust which is separated in the manipulation of the salicylic wadding. Wadding which does not produce this effect does not contain a sufficient quantity of free salicylic acid. Also the hands of the surgeon suffer, becoming rough, as from the use of carbolic acid.

Two ways were open to me for storing a sufficient quantity of the antiseptic for a considerable time in proximity with the wound without renewing the dressing. I could either apply a wadding-dressing highly charged with salicylic acid, and in

proportion as blood and exudations showed themselves on the surface of the dressing, cover this with fresh layers of salicylic wadding, or I could always add fresh quantities of salicylic acid to this wadding-dressing by irrigating it with salicyle water.

Since I could foresee in reference to the dry dressing, that when it was left on for a long time, the layers of wadding lying next the wound must become impervious to the exudations from filling up of its interspaces, it was to be feared that the supply of the antiseptic would not suffice for the fluids thus retained between the dressing and the wound. But such a deficiency of the antiseptic at the point where it was most needed could be remedied by irrigating the dressing with salicyle water, and since this to some extent counteracts at the same time the retention of wound-fluids, I began my experiments with this moist form of dressing, although it has many inconveniences for the patient and the attendants in comparison with a dry dressing. In the course of the experiments, it appeared for what cases the dry salicylic dressing sufficed, and for what cases the moist dressing with irrigation was fittest, and to these must be added, as third variety, the open treatment with salicylic irrigation, similar to that followed in Bardeleben's wards with carbolic acid. Further on I will mention the indications hitherto determined for each of these three forms of dressing.

Before reporting upon the first experiments made with salicylic acid, I will touch upon two questions, to the answers to which I can contribute but little; the one pertains to the microscopical contents of the wound-secretions, the other to their chemical decomposition under the salicylic dressing.

As regards the microscopical contents, the difficulty lies, not in the examination of the cellular forms, and of the fragments of tissue, but in the granules which exist from the smallest diameter recognisable, even with powerful immersion-lenses, up to the size of the larger spherical bacteria. I have not found that they differ from those met with in wounds treated by the open method. They are partly difficult to distinguish on account of weak refraction, and exhibit partly the strong refraction of fat. They present themselves in largest quantity, so as to cover almost entirely the whole field of the microscope, during the first days. They almost all have a lively molecular movement, which is frequently so strong that I mistook them at first for

bacteria. With this tremulous movement, however, they do not change their position, except when a current exists, which they follow unresistingly, however weak it may be. This circumstance appears sufficient to prove that their movement is a molecular one. A part of these is dissolved by acetic acid; of the remainder, potash lye, also sulphuric æther, after long contact and when warmed, dissolves another part, but some resist these solvents. Are these, therefore, to be regarded as bacteria, or are they perhaps inorganic deposits in a molecular form? I have not seen chaplets of any length; two or more sometimes hang together, swimming free or situated upon flaky detritus, and these grouped granules are in molecular motion. In a later interval of time, if the dressing has remained on for several days, I also frequently observed very fine rods in a state of oscillation, which disappeared in acetic acid or potash lye, and are no doubt partly threads of fibrin, partly crystals of fat. Amongst the rod-like corpuscles I sometimes found pretty large ones of a yellow colour, and joined together to the number of three or four in the long direction. Concerning these, I believe that I have observed them to result from the action of the salicylic acid upon the red corpuscles of the blood. The blood-discs become clear, as already mentioned, in the centre, while the sharply defined outer zone remains distinctly yellow. If we overlook the clear colourless centre, we may take them for rings, each of which has indentations at pretty regular distances, breaks up at one of these indentations, and separates itself gradually in the form of a jointed chain from the central part. These forms likewise possess molecular movement. I do not remember having seen them with the carbolic dressing, but in other respects the microscopical contents in both kinds of dressing agree with those of ordinary healthy wound-exudation. But Dr. H. R. Ranke* has now frequently found in the clinical wards at Halle, and even during the favourable course of healing in wounds with the carbolic dressing, distinctly characterised and further developed forms of bacteria, and it remains to be seen whether something similar will also be found with the salicyle dressing. In the case of the carbolic dressing it is now certain that when unchanged for a long time the amount of

* Bacteria-vegetation under the Lister dressing. Centralblatt für Chirurgie, 1874, No. 13.

carbolic acid becomes too small by evaporation, and when once the first critical period of healing is passed, the appearance of bacteria may, in most cases, be innocuous. With the moist salicyle dressing bacteria cannot thrive at all. The constant renewal of the antiseptic prevents this. The wound-secretions are here placed under the same conditions as if they were exposed to the air in a vessel with excess of salicyle water, in which case no trace of a development of bacteria is observed. They might more easily develop themselves when a dry salicyle dressing is left on for a long time, but I have not been able to demonstrate their presence after fourteen days. So much for the microscopical contents, which of course does not preclude the presence of bacteria, but which speaks against a further development, and an active participation by them.

It can scarcely be doubted that a chemical change takes place in the wound-secretions under the salicyle dressing. Even the precipitation of albuminates is no doubt connected with such a change. When the dry salicyle dressing has been left on four or five days, or the moist one a longer time, a rancid or cheesy smell is recognisable. The presence of organic ammoniacal combinations betrays itself by the production of a cloud on the approach of a glass rod moistened with dilute hydrochloric acid. It might be thought that this reaction was caused by admixture of the secretions of the neighbouring skin contained in the dressing, and they may take part therein; but we also obtain this reaction if we allow pus mixed with salicyle water to stand for some time in a vessel. Now and then, even with a favourable course, we find that Lister's protective, which contains lead, when laid between wound and wadding, is coloured black, and in these cases, silver also becomes black. This sulphur reaction cannot, however, result from sulphuretted hydrogen, since this would be recognisable by its smell. It is probably a question of products of organic decomposition, which contain loosely combined sulphur, and which, like oil of mustard (*sic*), give it up to metals. Only in a few cases, *e.gr.* in Table 1, No. 2, does sulphuretted hydrogen appear to have been formed in consequence of insufficient antiseptic action.

What part salicylic acid plays in the chemical changes in the wound-secretions I was unable to ascertain. If we test, layer by layer, the quantity of salicylic acid in a dry salicyle

dressing which has remained on for some time, and is soaked with secretions, the reaction with chloride of iron becomes weaker the deeper we go, and not unfrequently fails altogether in the pus confined between wound and dressing. If salicyle wadding, soaked in pus, is placed in a vessel partly filled with salicyle water, but so that the wadding shall reach beyond the surface of the fluid, the salicyle reaction gradually ceases in the projecting part, on which a luxuriant fungus vegetation appears, while the fluid covering the floor of the vessel, and containing free salicylic acid, remains clear and free from smell. Whether this cessation of the salicyle reaction occurs because the acid takes part in the decomposition, or because it unites with the organic combinations of ammonia which are set free, I am unable to determine.

I suspect that this conversion of the secretions takes place independently of the atmospheric ferments, and, in fact, from the access of the oxygen of the atmosphere. That a conversion takes place with absorption of oxygen when atmospheric ferments are excluded was already established by Helmholtz* in reference to fluids capable of decomposition as early as 1843. In our case, the modifying action of the salicylic acid would have to be taken into account.

In the case of subcutaneous wounds, neither the atmospheric ferments nor the oxygen of the atmosphere have access; in that of the usual treatment of open wounds both have access; in that of the antiseptic treatment of open wounds the ferments are excluded, but not the oxygen, and to this comes also the presence of the antiseptic substance in the neighbourhood of the wound. The chemical conversion of the fluids of the wound will necessarily differ in each of these three cases, and therewith its reaction upon the organism be different. A fuller knowledge of these conversions and a classification founded thereon will long remain a mere devout wish.

In the third part the reader will find all the cases treated antiseptically during the last ten months arranged in a tabular form, and of these I will here give those in which salicylic acid was first employed. In the description of them I will confine myself to that which appears to me necessary to give an intelligible sketch for judging of the local treatment of the wounds.

* Müller's Archiv, 1843, p. 458.

All the operations were performed, when possible, by Esmarch's bloodless method, after previous careful disinfection of the neighbourhood of the operation, under cover of antiseptic spray, and with disinfected hands and instruments. Especial importance was attached to checking the arterial hæmorrhage before dressing the wound, for which purpose Lister's carbolised catgut was employed exclusively. In cases of amputation, with the exception of the upper arm, I prefer that method of forming the flaps by which one larger flap is formed, which, when the stump is placed in position, lays itself by its own weight upon the surface of the wound, and one smaller flap which projects somewhat over the posterior edge of the surface of the stump. For the overhanging flap I take from the underlying fascia and muscular tissue a layer not too thick to deprive the flap of its pliability. This is not done to obtain a better cushion, but to secure the nutrition of the flap itself. The horizontally-placed network of vessels from which the perpendicular skin-branches arise, lies partly close upon the fascia, and its conservation appears to me essential to the vitality of the flap. To include in the flap a thin layer of the muscular tissue placed beneath the fascia appears desirable for insuring the nutrition of the fascia. For the upper arm alone I have retained the circular incision "*à deux tems.*" Sutures and drainage were used in almost all the cases.

I.—Osteo-sarcoma of the Leg.—Amputation of the Thigh.—Recovery.

Francis R., æt. 17 (Table I. No. 1), emaciated and weakly, is suffering from an osteo-sarcoma the size of an ordinary head on the left leg, which commenced a year ago and is increasing rapidly. Glands unaffected. Amputation of the thigh with overhanging flaps—sutures. In each of the two corners of the wound an india-rubber tube reaching to the bone was made fast. The stump was covered with a layer of wadding as thick as the finger, weakly charged with salicylic acid, equally and slightly pressed on with a gauze bandage. Salicyle irrigation, on an average, ten drops to the minute. This water assumed a violet-gray colour during the first few days by the washing out of the blood contained in the wadding.

No fever, pain, or swelling.

The first change of dressing, on the sixth day, showed the stump to be free from swelling and redness, as immediately after the operation. Edges and cavity of the wound closed as far as the points of drainage. At single points a little fatty tissue lay free between the sutures, which was but slightly turbid and appeared almost as if freshly cut. The sutures were removed and a moist dressing again applied. On the thirteenth day the dressing was again changed, and the drainage-tubes removed. From that time, dressing with ointment. On the twenty-seventh day the points of drainage were also firmly cicatrised.

II.—Osteo-sarcoma of the Forearm.—Amputation of the Forearm.—Interrupted Course.—Recovery.

W—, æt. 48, wife of a weaver (Table I. No. 11), underwent amputation for a large osteo-sarcoma on the upper third of the left forearm. Operation and dressing as in the former case. Free irrigation with salicyle water because the dressing became coloured with blood soon after the operation. The patient is very restless, sleepless, vomits much, and although there was no increase of temperature, it appeared necessary to examine the wound on the third day. We found the skin raised up and tense from congealed blood; the sutures were removed and the clots, which had no bad smell, taken away, the wound, in which some capillary hæmorrhage was going on, dressed afresh with salicyle wadding, and the skin covering the stump brought into a good position with a gauze bandage instead of the suture. Our hope was, that as there was still no fever and the stump was not swelled, union by the first intention might yet take place. But already on the evening of the same day, the patient being very restless and the dressing again soaked with blood, the temperature rose to 103.6°. On renewing the dressing, the stump was found to be somewhat swollen and red, and the discharge no longer free from smell. On the evening of fourth day, temperature 104°, pulse 124, patient restless and delirious. On the morning of the fifth day, temperature 102.5°.

The open treatment with salicyle irrigation was now adopted

and the inflammation and fever gradually decreased. On the thirty-ninth day the stump was almost healed and the patient completely restored.

No doubt I continued the attempt to produce healing by the first intention too long after the first failure, but fortunately without further evil consequences than a temporary deterioration of the state of the wound and of the general condition of the patient. That in spite of the retention of the blood in the wound during the first three days no fever occurred, and that this blood was still free from smell on the third day, may, I think, be put to the account of the salicylic acid.

III.—Neuralgia and Paralysis of the Leg after cure of Fracture. — Amputation of the Leg. — Interrupted course.—Recovery.

H. M., æt. 43, mechanic (Table I. No. 5), had had in 1873 a direct fracture of the right thigh, and a compound fracture of the left leg. The former healed satisfactorily, but, on removing the dressing from the left leg, the fracture was, indeed, found to be well united, but the foot at once assumed the position of a paralytic club-foot. It was found, in fact, that the peroneal muscles, as well as the extensors of the toes, were paralysed, and the patient complained of severe pains on attempting to stand, and even when the plantar surface was touched, which after some time were accompanied by twitching in the non-paralysed muscles of the leg. The skin was mostly of a bluish-red colour and cool to the touch. Baths, electricity, orthopædic bandages were of no use. Although the callus did not present any irregularities, the possibility of compression or dragging of a nerve at the point of fracture existed, and I decided upon laying bare the deep peroneal nerve at that point, but the nerve was found to be perfectly normal to the eye and in no way involved in the callus. The wound was treated by Lister's method, and although the long and deep incision-wound did not heal by the first intention, no spreading suppuration occurred. Notwithstanding the restlessness of the patient, healing went on regularly and without fever, by granulation. The paralysis continued as before, the neuralgia and twitching increased from month to month, and every mode of treatment

remained unsuccessful. I at last yielded to the patient's wish and amputated above the point of fracture, although perhaps even amputation would not suffice to remove the seat of the evil. The operation was performed in the manner already described, and a similar salicyle dressing applied. When the patient awoke from the effect of the narcotic, twitchings began in the thigh which threw the stump about. The pocket formed by the skin was naturally filled with blood, and already on the next day, therefore, I removed the middle suture, but without removing the congealed blood, which gradually came away with the occasional returns of hæmorrhage. The attempt to produce rapid union having failed, the wound was treated by the open method, with occasional irrigation or moistening with salicyle water. Morphia, chloral, and warm baths were tried with little or no effect for keeping the stump at rest; more efficacious was a massive plaster of Paris case which extended round the pelvis and thigh as far as the knee, and was fastened to the bed with straps. By this means the throwing of the stump about was at least prevented; gradually, from the eighth day, the twitchings decreased, and on the twenty-fourth day the stump was healed in its deeper part. When the remaining sutures were removed, a good granulating surface presented itself, which gradually cicatrised.

If we bear in mind that the local conditions for the healing of a wound were here of the most unfavourable character, it appears remarkable that no inflammatory swelling of the stump, no mortification of the skin of the flaps, no exfoliation of the sawed surface, no subsequent arterial hæmorrhage and no fever occurred. It appears reasonable to conclude that the antiseptic effect of the salicylic acid upon the blood and secretions retained in the wound, in combination, of course, with the free escape afforded by the removal of the suture, brought about the favourable course of the healing process.

### IV —Amputation of a Hand rendered useless by an injury —Interrupted course.—Recovery.

In this case the amputation-wound was washed out with a solution of salicylic acid 1 : 10, and healing without drainage attempted. Both proved to be deleterious, and a profuse discharge and retention of wound-secretions occurred.

St. F., æt. 49, labourer (Table I. No. 12), had had one of his hands rendered utterly useless in a severe machinery accident five months previously. There were fistulous, suppurating openings and constant pain in the hand. Several phalanges and metacarpal bones were broken and the fractures badly healed, the joints of the fingers were almost all anchylosed, the skin and cellular tissue thickened, resembling a board, and bluish-red, and the tendons of the extensor and flexor muscles ended in a broad cicatricial mass on the palm and back of the hand, where, according to the patient's account, large wounds, with loss of substance, had existed. On the 21st of May the hand was exarticulated and an overhanging flap formed out of the somewhat infiltrated, livid dorsal skin. The wound was finally washed out, as an experiment, with salicyle spirit 1 : 10 and closed exactly by suture without drainage. On the next day already it was necessary to remove the middle sutures on account of distension from retained secretions and blood, and the wound now went through the slow process of granulation, while a narrow strip of the edge of the overhanging flap became detached. There was no traumatic fever, but the patient had slight erysipelas from the second to the eleventh day, which commenced at a distance from the wound, on the upper arm, and slowly, at a temperature not exceeding 102°, extended to the shoulder and then died out. On the thirty-ninth day, when the patient was discharged by his own wish, a narrow granulation still remained.

V.—Necrosis of the Shin-bone.—Amputation of the Leg.—Recovery.

Str. M., a girl, æt. 11 (Table I. No. 6), was brought in in an extremely emaciated condition and nearly dead. In addition to periostitic suppuration of the right elbow, the chief evil consisted in a sequestration of the whole diaphysis of the left shin-bone, the leg was bent at an acute angle at the knee and pressed against the thigh, and had fistulous openings in it at various points. From one of these openings projected the upper end of the large sequestrum, which was removed without difficulty.

The cavity left by it was treated in the usual way, and the attempt made at the same time to extend the leg gradually, but with partial success only, because there was not a sufficiently

firm bony connection between the upper and lower epiphysis, and indeed separation of the upper epiphysis soon took place, which remained in its acute angle while the rest of the leg could be extended.

The attempt to scrape this bent portion of the epiphysis and then to unite it with the scraped extremity of the upper diaphysis proved useless. The longitudinal incision made for this purpose from the tuberosity of the tibia downwards, showed that a piece of bone, scarcely so thick as a lead pencil, lay at the bottom of the cavity. Nothing remained therefore but amputation, which I at once performed by converting the long incision already made into an oval incision, the point of which lay above and in front and the base below and behind. The head of the shin-bone, which lay at the bottom of the funnel-shaped opening and was filled with spongy granulations, was scraped, the hæmorrhage carefully stopped, the edges of the skin sewn longitudinally, and a drainage-tube placed in the lower corner of the wound and reaching to the bone. The stump was dressed with moist salicyle wadding and kept moist by occasional irrigation with salicyle water. This was on May 22. On May 24, when the dressing was changed, there was no swelling and but little discharge. The patient never had fever, and the remaining suppuration having ceased, was discharged with crutches on August 5.

VI.—Traumatic Gangrene of the Hand and Arm.—Amputation of the Upper Arm, with high fever.—Slow recovery.

G. K., æt. 65, mechanic (Table I. No. 10), had the skin, tendons, etc., of the right hand extensively torn by machinery at the end of May, and was taken in on May 31, with traumatic gangrene of the hand, which, in spite of incisions and antiseptic irrigation, spread, with high fever (103°—104°), to the forearm and caused œdema of the whole extremity as far as the shoulder, with considerable swelling of the axillary glands. The amputation of the upper arm, on June 7, had to be performed in its upper third. Circular incision *à deux tems*, with ample supply of skin, sutures, drainage, moist salicyle dressing, irrigation. Dressing changed on fifth day, œdema not entirely

gone, edges of skin adherent, copious suppuration at bottom, which diminishes very slowly, drainage-tube removed on tenth day, but there are still two fistulous openings on thirteenth day. A small piece of skin of edge of wound also sloughed off.

This slow course is no doubt to be attributed to the advanced age of the patient, and to the œdematous state of the limb at the point of amputation. The general symptoms were favourable, since the fever ceased from the day of the operation, and the patient recovered rapidly.

In the two next cases of amputation, Nos. VII. and VIII., the result was fatal.

VII.—Pseudarthrosis of the Thigh.—Amputation at the point of Pseudarthrosis.—Acute suppuration of the Shoulder-joint.—Death.

W. Gustav, æt. 53 (Table I. No. 2), was admitted February 26, 1874. Fourteen weeks previously, indirect fracture of the left thigh, a little below the middle. Capsular bandage, no consolidation. The patient is very emaciated, anæmic, with wrinkled skin and flaccid muscles. Left thigh hard, soft infiltration of leg. The limb shortened 2.4 inches, the upper end of bone presses in a point against the skin, fracture very moveable. Subcutaneous tearing of the fibrous attachment by bending and turning; pulleys reduced the shortening to 1.2 inches. To have extension with adhesive plaster and good diet. No improvement, either at the point of fracture or in state of nutrition; on the contrary, increasing debility and œdematous swelling. Intercurrent purulent discharge from middle ear.

After four weeks more, *i.e.* after nearly eight months' confinement to bed, *amputatio femoris* at the point of pseudarthrosis with overhanging flaps of skin and muscle. A thin layer of muscle was found lying between the broken ends of bone and the posterior surface of the atrophically-pointed upper broken end was deprived of its periosteum to a small extent.

Operation and dressing as in preceding cases, with drainage-tube reaching to the rough portion of bone. Dressing renewed on fifth day, sutures removed; up to that period, highest

temp. 101.5°, neither swelling, redness, nor pain. Appetite, sleep, and appearance of patient improved.

On the eighth day, the brownish discharge running from the drainage-tube, 3—4 teaspoonsful in twenty-four hours, had a bad smell resembling that of maceration of bone and coloured silver black, still more so on the tenth day, on which the moist dressing was changed, on account of extensive loosening of the epidermis, for dry salicyle dressing. Up to the fifteenth day, continued good condition of the patient, on that day the temp. 104°, with violent pain in the rapidly swelling shoulder-joint, remitting fever with high temperatures, rapid sinking of the powers; antiseptic laying open and drainage of shoulder-joint on July 2.

Death on July 4, on twenty-third day after amputation, on eighth day after commencement of suppuration in shoulder-joint. In the stump no change worth recording occurred during this latter period.

The post-mortem showed everything healed about the stump, with the exception of a suppuration-canal which led to the rough portion of the bone, no peeling periostitis, no osteo-myelitis. There were also no changes appertaining to embolic pyæmia in the veins, or in the parenchyma of the lungs. The suppuration in the shoulder-joint had commenced in the synovial membrane.

The suppuration of the shoulder-joint, commencing acutely with a previously fever-free course of the wound, cannot be attributed to embolic pyæmia, for such cases, two of which are given in the Table of the Second Division (Nos. 70 and 87), differ from it both clinically and anatomically; neither have they anything in common with the pyæmic suppuration of the joints and serous sacs. On the other hand, a causal connection between the suppuration in the wound and the secondary suppuration cannot be denied, and these cases remind us of the abscesses which occur in connection with acute articular rheumatism, pneumonia, etc. Moreover, that the disinfecting process did not suffice in this case, probably from insufficient escape on account of the depth of the wound, is seen from the occasional bad smell of the pus, and from its sulphuretted hydrogen reaction. A second drainage opening ought to have been made, posteriorly, opposite to the end of the bone.

VIII.—Gangrenous Ulcer of the Leg.—Amputation of the Thigh.—Gangrenous breaking up of the Surface of the Wound.—Death from Exhaustion.

Fr. Gl., æt. 35, carpenter's wife (Table I. No. 3), was admitted July 1, 1874, for a gangrenous ulcer on the left leg and foot. The ulcer, which had existed for years, included the lower fourth of the leg and tarsal region of the foot. At the bottom of the gangrenous soft parts we see the bones lying bare and the joint laid open. In a few days the connection between leg and foot was reduced to the tendo Achillis, the division of which sufficed for their separation. In spite of the permanent water-bath, the gangrenous process advanced upon the leg, either in consequence of the extreme debility of the patient, who had passed the last few years in great misery, or perhaps on account of some syphilitic complication. A scar on the thigh and two spots upon the tongue resembling mucous papulæ appeared to justify the administration of iodide of potassium. Of course every attempt was made to raise the strength by good food. But as this did not succeed, and after several evening temperatures of 102.5° and more, the temperature went down to 95°. On July 1, the time appeared to have arrived for making a last attempt to save the patient by substituting for the large gangrenous surface an amputation-wound. On July 16, amput. fem. in its lower third. Esmarch's bloodless method—salicyle spray—overhanging flap—catgut ligatures—wound sutures—drainage—moist salicyle dressing, salicyle irrigation.

Hæmorrhage at most 1½ ounces. After the operation, pulse scarcely to be felt, 88, temp. 94°, patient takes no nourishment, intense pallor, apathy. Injection of twelve ounces of defibrinated human blood into the ven. med. ceph.—pulse stronger, 64, lips become red, the look brighter, temp. about 97°, with oscillations, and remains normal from the second day after the transfusion. (Although the transfusion was effected without chloroform, with the patient's full consciousness, she showed no sign of pain during the operation, and had no recollection thereof afterwards, which appears to me attributable to the state of "minima vita" in which she found herself).

When the dressing was changed on the fourth day after the amputation, the stump looked well, the edges of the skin appeared healed, there was no swelling, no smell, and but little discharge. On the eighth day it was seen that the wound was not closed at its deeper part, the sutures were removed, the lips of the wound partly opened and an ill-smelling, brownish fluid removed. From this time the dressing was changed daily and the wound washed out with salicyle water. In spite of this, the surface of the wound became covered with a grayish-green coat resembling that presented by the original ulcer, the edges separated and the skin became partly gangrenous. Meanwhile, the appetite continued good until the twentieth day and the patient remained free from fever. From the twenty-first day the appetite decreased, the temperature sank quickly to 95°—92°, and a fresh transfusion had a transient effect only upon the pulse. Death occurred on the twenty-seventh day after the operation.

The post-mortem gave hæmorrhagic infarctus of the right lung, recent and no doubt attributable to the last transfusion, the surface of the wound in a state of soft breaking up to a slight depth, the medulla reddened. In the femoral and iliac veins fresh thrombi. The post-mortem furnished no evidence of syphilis.

My view of the case is, that the extreme debility to which the patient was reduced caused the gangrenous breaking up of an originally simple ulcer of the leg just as it interfered with the healing of the amputation-wound. The bad smell which set in with the breaking up of the surface of the wound is explained by the fact that the salicylic acid acts upon the surface of the wound only and does not reach the tissues lying in the lower gangrenous layer.

The first experiment whether the dry salicyle dressing also acts sufficiently antiseptically was made on an amputation-wound of the thigh.

IX.—Osteo-sarcoma of the Leg.—Amputation of the Thigh.—Recovery.

Fr. Ernestine, æt. 13 (Table I. No. 4), had amputation of the thigh with formation of an overhanging flap performed in

the inferior epiphysis of the femur, on account of a large osteo-sarcoma of the fibula. The wound was sewn up this time with carbolised catgut, in each corner a drainage-tube introduced reaching to the bone, the line of suture covered with protective, and the stump then enveloped in salicyle-wadding, gently and equably pressed on by means of a gauze bandage. There was neither pain nor fever. When the dressing was first changed on the tenth day, there was a small amount of discharge between the wound and dressing at the points of drainage, the wound was healed as far as these points, and the sutures mostly divided by absorption. The drainage openings closed in a few days.

X.—Large Subfascial Abscess after Articular Rheumatism.—Incision.—Recovery.

The second experiment was made with a large abscess which had developed itself in connection with acute articular rheumatism, with subfebrile temperatures, see Table I. No. 61. The abscess was situated subfascially in the fossa infraspinata of the right shoulder-blade, was opened for emptying and draining at two opposite points, washed out with salicyle water, drained and dressed dry with salicyle wadding. The pus withdrawn amounted to 500 cubic centimeters. The cavity of the abscess healed by the first intention and eighteen days after the operation, the drainage openings also closed.

Of the series of experiments of the first six months two cases may be given here which, although the results were unfavourable, appear to show that salicylic acid is capable of preventing the dangerous forms of decomposition.

XI.—Fracture of the Leg.—Plaster of Paris Dressing.—Perforation of the Skin in consequence of Delirium Tremens.—Advancing Sepsis at point of Fracture.—Amputation of Leg.—Advancing Suppuration of Amputation-wound.—Amputation of Thigh.—Hydrothorax.—Death.

W. Wilhelm, æt 46, typefounder (Table I. No. 7). Subcutaneous, comminuted fracture of the left ankle-joint from being run over. Plaster of Paris dressing.

1—9 day.—The first four days passed favourably. On the fifth, in spite of prophylactic doses of morphia, furious delirium tremens came on, in consequence of which the upper end of fractured shin-bone pierced the skin, which was not observed at first. On removing the dressing, commencing sepsis was found which, notwithstanding the Lister antisepsis, advanced rapidly, evening temperature 103.75°. On eighth day, amputation of the leg in its upper fourth, with continuance of delirium, on the ninth day, evening temperature 105°.

10—71 day.—Moist salicyle dressing with salicyle irrigation. The delirium decreased, but there was almost constant restlessness of the whole body with impaired consciousness. The temperatures decreased gradually so that the mornings were mostly free from fever and the evening temperature did not exceed 102°. The constant movement of the stump with great local irritation caused peeling periostitis along the heads of the tibia and fibula, in the vicinity of which the skin was pierced at several points. The antiseptic action proved insufficient against the deep suppuration. The pus assumed a bad smell and coloured silver black. Nutrition insufficient.

72—98 day.—Subfebrile and febrile temperatures alternately. The moist dressing was changed for dry salicyle dressing. The knee-joint swelled, there was breach of surface at various points, from which it was at first doubtful whether the pus came from the periostitic suppuration or from the joint. When the latter became clear, with constantly decreasing power, and œdema of the left leg, amputation of the thigh above the middle was performed on Nov. 10.

98—123 day.—Amputation with overhanging flap. Dry salicyle dressing. Patient remained free from fever, with improved appetite. There was increase of strength, the œdema of the left leg decreased, and the consciousness became perfectly clear. The amputation-wound healed, not by adhesion indeed, but by a healthy process of granulation. On the 14th, 18th, and 21st day after the amputation, slight hæmorrhages from the granulating medullary canal without demonstrable cause, followed each time by increase of temperature to 100.5°.

123—147 day.—Although the patient remained free from fever, he lost ground from this time. First the appetite failed, then increased œdema of the left leg showed itself and œdema of

the scrotum and stump set in, extending to the pelvic region. Both pleuræ became filled with exudation, producing so much dyspnœa that tapping was performed on the left side on December 19 and 22. A yellowish, opalescent fluid, rich in albumen came away, the first time 2,300 cubic centimeters, the second time 1,200. A transfusion of 300 cubic centimeters of defibrinated human blood on the last day was followed by a transient improvement only. Death on December 30.

*Post-mortem*, December 21, 1874.—Amputation-wound of right thigh about four inches below the great trochanter. Stump of bone covered with a thin layer of flabby granulations, under which red medulla not in a state of suppurative inflammation; in the head of the femur normal, yellowish medulla. Wall of the art. fem. dextr. thickened, the pointed blind end filled with thrombus in a state of commencing vascularisation. Vena fem. dextr. empty of blood, filled above Poupart's ligament partly with a marantic thrombus softened at the centre. Fresh clots upwards as far as vena cava. Extensive hydrothorax on both sides with almost complete compression of both lungs. In the compressed lung-tissue, numerous recent and older cheesy deposits of tubercle, in the apex of the left upper lobe, a small cavity filled with cheesy masses. Fresh miliary tubercles in both pleuræ, single fresh ecchymoses on right diaphragmatic pleura. Left ventricle of heart firmly contracted, almost empty of blood, right ventricle flaccid, filled with fresh coagulum. Slight fatty degeneration of liver. Kidneys not enlarged, firm; pyramids hyperæmic with venous blood, cortical substance pale; in cortical substance of left kidney, a fresh, blood-red, wedge-shaped nest (embolic?), older cicatrices beneath the capsules of both kidneys. In the mucous membrane of the stomach, about ten small, mostly round ulcers covered with bloody mucus (embolic?), fatty degeneration of the muscular fibres in jejunum and ileum.

All attempts to obviate the excitement which remained after the passing off of the furious delirium, and thus to place the stump at rest, having failed, in the place of cicatrisation, suppuration naturally set in, which eventually involved the knee-joint. The amputation of the thigh, which appeared, under the circumstances, to be the only means of saving life, seemed at first to succeed so far as to give us some hope. But when

hydrothorax occurred rapidly and continuously from the 123rd day, the last hope faded away, and even transfusion could no longer be of any avail. That the hydrothorax was connected with recent, acute, miliary tuberculosis of both pleuræ, and of the lung, and that old deposits of tubercle existed in both apices, we first learnt from the post-mortem. The capillary hæmorrhages of the pleura and cortical substance of the kidney resulted from the transfusion. It is worthy of remark that no septic changes took place in the medulla, although it lay so long in suppurating wounds.

XII.—Compound Fracture of both Legs.—Amputation of them both.—Delirium Tremens.—Partial Destruction of the Skin.—Advancing Suppuration, especially on the left side.—Metastasis of the Suppuration to the left Knee-joint.—Amputation of left Thigh.—Death from Exhaustion.

E. Ludwig, æt. 64, railway driver (Table I. No. 8), had been driven over. Double amputation with overhanging flaps. Moist salicyle dressing.

1—5 day.—No fever, temperature only once 100.5°.

5—10 day.—Furious delirium tremens, repeated parenchymatous hæmorrhage, extensive sloughing of the skin of the flaps. On sixth day, change to open treatment with salicyle irrigation. In the night from September 1 to 2, a transient attack of collapse, with weak pulse and great dyspnœa, occurred.

11—48 day. The wounds are becoming clean and covered with granulations, but these are flabby and pale, cicatrisation from the edge does not go on satisfactorily, and the sawn ends of bone are deprived of their periosteum.

49—92 day.—Cicatrisation is not effected, the exuberant granulation goes on deep in the wound and under the skin, but passes into suppuration on the surface, especially on the left side, in consequence of which the left knee-joint suppurates. In the beginning of this period (October 9), a second, dangerous attack of dyspnœa occurred, which was connected this time with pleuritic stitch, pleuritic friction-sound on the right side, and a temperature of 102°. Dyspnœa, fever and pain passed off, but dulness on percussion remained.

93—100 day.—Amputation of the left thigh on the 93rd day seemed, at first, to promise improvement, but death occurred on the seventh day after the operation, the temperature having reached 102°, as a maximum.

It is to be mentioned also for all the five periods that the patient, with little interruption, and at occasional temperatures up to 101°, showed symptoms during the whole time he was under treatment of catarrh of the stomach, by which the administration of sufficient nourishment was rendered impossible.

The examination of the stump of the leg amputated November 23 showed, apart from the synovial suppuration, a 1—2 inches long, partly adherent thrombus of the popliteal artery, the walls of which were highly atheromatous, and partly chalky.

*Post-mortem*, December 1, 1874.—Normal amputation-wound on right leg and left thigh. Medulla normal on section. Circumscribed, suppurating pleurisy to the extent of a small fist on the posterior part of the right lower lobe. Circumscribed abscess of lung as big as a cherry. Fatty liver. Spleen slightly lardaceous. Kidneys healthy. A jagged ulcer on the pylorus, a square inch in size, extending to the sub-mucous membrane, several small ulcers in the duodenum, all smeared with blood. Considerable quantity of blood in small intestines.

Death occurred from exhaustion, accelerated by intestinal hæmorrhage. The exhaustion was caused partly by the insufficient nourishment dependent upon the catarrh of the stomach, partly by the long continued suppuration. The absence of cicatrisation and the advancing and constantly softening exuberant granulations may be attributed to the atheroma of the arteries, for on the one hand, the growth of healthy granulations depends upon a normal condition of the vessels, on the other the retrograde, cicatricial metamorphosis of the granulations is only to be expected with collateral equalisation of the disturbed circulation. Extreme atheroma is preventive of both. To the abscess in the lung we may refer the attack of collapse on September 1, while the violent movements of the stump occasioned by the delirium tremens may have brought about the passage of an embolus from the veins of the stump into the lung. The consequent abscess, gradually reaching the serous covering, may have caused the pleurisy on October 9, whose product was found at the post-mortem as a circumscribed exu-

dation. Neither the history of the case nor the appearances met with at the post-mortem justify us in attributing this circumscribed abscess to pyæmic infection.

Amongst the twelve cases given, there are four amputations of the thigh with fatal result, and in case 2 of amputation of the forearm, case 3 of amputation of the leg, case 4 of amputation of the hand, and case 6 of amputation of the humerus, the course run was not brilliant. The three good cases, of amputation of the thigh (cases 1 and 9), and of amputation of the leg (case 5), occurred in children and are, therefore, not of great importance. Also the case of abscess (10) proves nothing, as an isolated case. I have, nevertheless, received the impression just from the cases ending unfavourably that the salicylic acid, even if it does not prevent all decomposition, and is accompanied in some cases by ill-smelling products of decomposition, yet prevents that form of decomposition which leads to pyæmia. At least in cases 2, 3, 11 and 12, the other preliminary conditions of pyæmia appear to have been present. Only a larger number of experiments could lead to a definite judgment, and since the results so far spoke in favour of a repetition of the experiments, they were continued.

## III.

Dr. Joseph, then assistant-surgeon in the Leipzig Hospital, published in 1868 our first sixteen cases of abscess treated after Lister, for the purpose of recommending the new method. He did not, however, meet with the expected recognition. A bitter frost fell upon the fresh aspirations of the youthful author. Such results, it was said, are attained in Marburg,* as also in Tübingen, without antiseptic treatment. Why not? Also of the cases which I am now about to give there is not one of those which went on favourably which might not have done so equally

* Roser, Archiv d. Heilk., 1868, Bd. x. p. 165, and v. Bruns, surgical practice, 1873, Bd. ii. p. 984. Both these critics have made the mistake of thinking that Dr. Joseph attributed to the Lister method a direct healing effect upon the diseased bony parts in cases of congestive abscess. Against this I must speak in his defence; he means only, as does Lister himself, that it is beneficial for the diseased bone if the fistula leading to it is protected against septic decomposition, which object may be attained by the Lister method. In this point he is no doubt right.

under any other treatment, and as regards the unfavourable ones, I must submit to the objection that another might have been more successful. What struck us at that time was the contrast between the previous state of things and the present, the extraordinary improvement which the Lister treatment had effected in *our* hospital in *our* cases of abscess, and we thought that such an improvement might also be acceptable elsewhere. Under the influence of this contrast, Dr. Joseph had expressed himself somewhat strongly in his description of the results and thus probably excited to contradiction.

Very unfavourable is the report which Dr. Güterbock* has published of the results obtained with the Lister treatment in the Bethany Hospital at Berlin. Apart from the fact that the Report pointed out sharply the technical defects of the method as then employed, it was found that it did not prevent, to any demonstrable extent, the occurrence of pyæmia or of hospital gangrene in that hospital. This was the worst thing that could be said of it.

Since then, several years have passed away. Lister, convinced of the truth of his principle, has gone on improving the process, and his improved process was made accessible to the German public in a very commendable manner by A. W. Schultze. Lesser's communication was also acceptable and the method has now found conspicuous advocates with us in Volkmann, Bardeleben, Socin, and others. Although the technical details may be modified, Lister's postulate "exclusion of the atmospheric ferments from the wound" will certainly never again be lost sight of.

The majority of hospital and clinical surgeons nevertheless still maintain a doubting, expectant and dissenting position, and at our last year's Surgical Congress I became convinced that it is desirable to communicate to the medical public in a comprehensive form the experiences obtained from this or that treatment in hospitals, so as to place it in a position to form an independent judgment of their respective practical values. Such a record must by no means give a selection of cases, but rather include all those treated by one and the same method, neither does it suffice to give the mere numbers.

Unfortunately, such comprehensive records are as fatiguing

* Archiv für klin. Chir. 1872, Bd. xiii.

to read as to compile, since it is a question almost entirely of our every-day work, but nothing more practical occurring to me, I determined, in the hope that other hospital surgeons would follow my example, to make the beginning and to tabulate the 160 cases treated antiseptically during the last ten months in the Leipzig hospital.

The Table begins with the April of last year, because the first experiments with salicylic acid were made in that month. Such a Table is capable of showing whether the method adopted can obviate incidental wound-diseases, whether it prevents advancing suppuration and ichorous secretions and facilitates union by the first intention, what form the wound fever assumes under it, how it stands with regard to after hæmorrhages, sequestration of sawn edges and bones laid bare, and what is the duration until the wound is closed.

In the appreciation of favourable results, the building in which they are obtained plays a very important part. I am prepared for the objection that we are indebted for what is good in our results, not to the antiseptic method, but to the newness and good condition of our new Jacob's Hospital. Placed in an elevated, free position at the south end of the town, and covering a large area, it is divided into two parts. The one part, formerly the orphanage, is as well suited as possible to the purposes of a hospital. There is room in it for about two hundred patients. The other part consists of twelve well-constructed barracks on the American system, for twenty-four beds each, and four slightly constructed ones for fifty to sixty beds each. The latter were erected in great haste in 1871, on account of a severe epidemic of small-pox, and since they stand close to our hospital, which has already proved to be too small, they serve as a reserve for slight cases and convalescents. They go to this day by the name of the "Small-pox Barracks."

In the main building, eighty beds are appropriated to the surgical department, of the good barracks it has seven, and of the small-pox barracks two at its disposal. This gives 348 available beds, and since the daily average seldom exceeds 240, the complete evacuation of each ward is often feasible.

The ventilation, especially in the barracks, is easily effected. In winter the fresh air is introduced warmed and the foul

air carried off by flues; each ward is supplied with warm and cold water, all the excreta of the whole establishment are rendered harmless at once by disinfection by Süvern's method, so that they cannot infect either the air or the ground. The inventory for 400 patients is entirely new, infected linen and bedding are at once removed, to be brought back only after thorough cleansing, and great attention is paid to cleanliness in other respects. The attendance upon the patients is, to a large extent, in the hands of experienced Dresden deaconesses, the establishment has its own dispensary, which is held to be one of the best, and whatever the medical staff desire in the way of nourishment is readily furnished by the civic authorities. In a word, it may be said that the arrangements of a large hospital are scarcely to be found more perfect anywhere in Germany. That Leipzic has spared no sacrifice to be the first to introduce the American barrack system on a large scale on this side the ocean will always be an honour to it.

Nevertheless, we are not free from incidental wound-diseases. From hospital gangrene, it is true, we have not to suffer; once only did it pay us a transient visit, and since the circumstances of that visit give food for reflection, I venture to communicate the details of it.

At the end of March, 1871, the recently furnished, new Jacob's Hospital was brought into use. The old furniture was not yet disposed of, and since it could not remain in the old Jacob's Hospital, it was brought to the new one and, for the moment, deposited in the two courts of the hospital which opened on the street. Here it lay heaped up for weeks in rain, snow, and wind, no pleasant sight for passers-by. One day it had disappeared and was said to have been removed to a more fitting place. A few days after, without being introduced from without, hospital gangrene in its worst form showed itself, to my no little alarm, in two barracks far distant from each other. We had had none such even in the old house since 1868. Each barrack has an empty ground-floor room from which the ward draws fresh air. Into these spaces I found the vanished furniture stuffed; tables, night-chairs, mattresses, etc., as if for an infection experiment on a large scale. What followed was a matter of course. Fortunately there were only two cases, neither of which proved fatal. After long exposure and much

washing, a part of the old furniture was selected for the small-pox barracks. Two of these now serve, as already mentioned, for slight surgical cases. It appears worthy of mention that the two worst cases of pyæmia—one after bleeding, and one when an operation-wound was almost healed—occurred in these barracks, as also a case of gangrenous erysipelas with fatal result after an operation for phymosis.

Erysipelas visits us with varying severity. In 1873 we had 75 cases amongst 1,902 patients; in 1874, amongst 1,921 patients,* the number of cases of erysipelas is not yet ascertained precisely, but is certainly much smaller. In reference to the antiseptic treatment, the two years do not differ. It is, I think, generally admitted that the antiseptic treatment is of no avail against this disease. Erysipelas occurs with carbolic as well as salicyle treatment, rarely or frequently, severely or mildly, and since the disease, whether as an isolated case or an endemic, is atypical, it cannot be determined whether the antiseptic treatment affects the number or the severity of the cases of erysipelas.

While the absence of hospital gangrene and the presence of erysipelas are not conclusive for the healthy or unhealthy character of our hospital, it stands otherwise with regard to pyæmia. On coming to Leipzig in 1867, I met with this disease and hospital gangrene to a serious extent in the old Jacob's Hospital. Long before American barracks were spoken of, it had driven my predecessor, Dr. Günther, into the open air with his patients, where he established himself as well as he could in a kind of skittle-alley. The state of things improved, indeed, after the erection of two new sheds, through the introduction of the Lister treatment and other measures, but unexpected cases were not wanting until the building was abandoned.

Things went much better in the new building, especially after the strict carrying out of Lister's method as described by A. W. Schultze.

* These two aggregates of patients in the surgical wards in 1873 and 1874 include for 1873 amongst 1,902 patients, 575 with contagious diseases of the sexual organs and 174 with itch; for 1874, amongst 1,921 patients, 563 syphilitic and 135 itch cases. The number of strictly surgical cases is thus reduced for 1873 to 1,153; for 1874, to 1,223. There were 518 accident cases in 1873 and 490 in 1874.

In 1873, out of 132 deaths, ten occurred from pyæmia; of these, four were imported, two, already mentioned, occurred in the small-pox barracks, the remaining four after amputation of the thigh, amputation of the humerus, enucleation of the index-finger under the Lister treatment, and one after a severe injury to the thigh, with exposed bone, under the open treatment.

In 1874, out of 106 deaths, five occurred from pyæmia, four commencing in the hospital and all treated by the Lister method; one of these was a case of compound fracture of the skull, one bad compound fracture of the leg with injury to the soft parts in the popliteal space, one severe compound fracture of both legs, and one after amputation of the humerus. Only the last case (Table I. No. 22), comes within the period of ten months of which I am reporting.

If, therefore, the number of pyæmic cases is not large, it still shows that in spite of the good state of our hospital, the preliminary conditions for the production of pyæmia are present, and that the building, of itself, does not afford sufficient protection.

Esmarch's bloodless method, that inestimable improvement of our operative procedure, must be spoken of here since it is a question of cases occurring during the last ten months. No doubt it has its part in the cases ending favourably; it saves blood, admits of a careful treatment of the surface of the wound, facilitates the tying of the arteries, and will thereby tend to prevent septic complications. But since, on the other hand, it renders operations possible which we dare not otherwise undertake on account of the weakness of the patients, and since the majority of these cases end unfavourably, it will have a bad effect upon the death statistics of those operated on if not on the number of incidental wound-diseases. Thus, in the case of the patient, Table I. No. 3, I should scarcely have decided upon operating without Esmarch's method, perhaps also not in Table I. Nos. 2 and 102, both of which ended fatally, and in No. 18 the amputation of the forearm would probably not have been attempted.

The first division of Table I. includes the cases from April 1 to September 30, 1874, the second division, with the cases from October 1, 1874, to February, 1875, has been added, because the printing of this treatise has been retarded until now, contrary

to expectation. This separation of the cases into two divisions shows how salicylic acid gradually took the place of carbolic. The 90 cases of the first division are half salicylic, half carbolic cases; of the 70 cases of the second division, 66 are salicylic, and only 4 carbolic cases. This separation also shows the withdrawal of the practice of washing out the wound with a strong solution of chloride of zinc or carbolic acid.

The third column of the Table contains the numbers of the wards. Nos. 1, 2, 3, 4, 5, 17 and 18 are first-class barracks; 13 and 14 former small-pox barracks, therefore second-class; the remaining numbers belong to wards of the main building (former orphanage). This was done for a future appreciation, so that in later times a judgment may be formed of the local distribution of infectious wound diseases, as shown by a greater number of cases. In like manner, for the purpose of facilitating later inquiries, the numbers in the chief register are given in the second column.

In the fourth column I have placed the initials of the respective assistant-surgeons. Upon them rests the heaviest portion of the medical service, numerous operations are performed by them; to them I am indebted for the notices concerning the course of the individual cases, and with ever lively interest and independent co-operation they have taken part in the carrying out of Lister's treatment with carbolic acid, and in the experiments with salicylic acid. These are the qualified surgeons, Dr. Burckhardt, Dr. Kaulfers, Dr. Credé, Dr. Humann, and for a short time as substitutes, Dr. Moldenhauer and Dr. Hörter.

The ninth and tenth columns are devoted to the commencement of the disease and the date of the admission of the patient, an especially important interval, since an aseptic course can only be looked for in the case of injuries coming quite fresh under treatment. On the complete closure of the wound, I have put down "cured," which naturally does not mean, however, that therewith all the effects of the injury, in so far as they are removable, have disappeared. To determine this date would, in most of the cases, have required observation long after the patients had left the hospital. That between the terms "cured" and "discharged" a longer time frequently passes than might be expected from the nature of the disease

depends, as every hospital surgeon knows, upon many causes. One patient has no proper place to go to, another is waiting for an artificial leg, etc. "Almost cured" is reported when only a superficial granulation of slight extent remains.

The fistulæ remaining after healed suppuration of the joints, resections, and empyema, are an evil residue, and even when the chief disease seems over, continue as independent affections. These I have reported as "cured with the exception of a fistula."

TABLE I.—LIST OF THE CASES TAKEN INTO THE SURGICA

*From April* 1, 187

FIRST DIVISION, FROM APRIL 1, TO SEPTEM

GREATER AMPUTATIONS, RESECTIONS, CO

| Number. | | Ward. | | Name of Patient. | Age. | Occupation. | Disease or Injury. |
|---|---|---|---|---|---|---|---|
| Current Number. | General Register | No. | Surgeon. | | | | |
| 1 | 690 | 5 | B. | R. Franz ... | 17 | Shoemaker | Osteo-sarcoma of left leg ... |
| 2 | 480 | 5 | B. | W. Gustav ... | 53 | Tinman ... | Pseudarthrosis of left thigh |
| 3 | 1067 | 18 | B. | Gl. Christiane | 35 | Labourer's Wife | Gangrenous ulcer of left foot leg. |
| 4 | 1445 | 1 | M. & K. | Fr. Ernestine | 10 | Bricklayer's Daughter | Osteo-sarcoma of right fibula |
| 5 | 29 | 4 | Cr. | M. Hermann ... | 42 | Farm Servant | Neuralgia and paralysis of left after healed fracture. |
| 6 | 99 | 1 | K. | Str. Marie ... | 11 | Bricklayer's Daughter | Necrosis of left tibia ... |
| 7 | 1224 | 17 | B. | W. Wilhelm ... | 46 | Typefounder | Compound fracture of right leg |
| 8 | 1304 | 3 | K. | E. Ludwig ... | 64 | Driver ... | Compound fracture of both leg |
| 9 | 1435 | 3 | M. & K. | Rr. Heinrich ... | 21 | Ry. Porter | Compound fracture of tarsus—wound of left foot and leg. |
| 10 | 918 | 4 | Cr. | R. Gottfried ... | 65 | Labourer ... | Compound fracture of right c and metacarpus, advancing grene. |
| 11 | 724 | 2 | B. | W. Christiane | 48 | Weaver's Wife | Sarcoma of left forearm ... |
| 12 | 847 | 3 | K. | St. Friedrich ... | 49 | Labourer ... | Compound fracture of carpus metacarpus—badly healed. |
| 13 | 1003 | 3 | K. | L. Gottfried ... | 53 | Labourer ... | Caries of left shoulder-joint |

No. 1, *vid. ante*, No. 1. Amput. for large sarcoma of leg.

No. 2, *vid. ante*, No. 7. Death from acute suppuration of right shoulder, com mencing in third week after the amputation.

No. 3, *vid. ante*, No. 8. Death from exhaustion.

No. 4, *vid. ante*, No. 9. First considerable case with dry dressing, which was le on for ten days.

No. 5, *vid. ante*, No. 3. Treatment disturbed by constant twitchings.

No. 6, *vid. ante*, No. 5. Favourable result.

No. 7, *vid. ante*, No. 11. Death from hydrothorax.

No. 8, *vid. ante*, No. 12. Death from intestinal hæmorrhage.

No. 9. Crushed by railway buffer. Amputation with overhanging flaps. ( account of ecchymosis of the tissues, the wound was left half open. Moist salic

WARDS AT LEIPZIG FOR ANTISEPTIC TREATMENT,

*o January* 31, 1875.

), 1874—(45 SALICYLE, 45 CARBOLIC CASES.)

OUND FRACTURES—SALICYLE DRESSING.

| mmence-ent of ease, or ate of njury. | Day of Admission. | Operation and Mode of Dressing. | Day of the Operation. | Result. | | Date of Discharge |
|---|---|---|---|---|---|---|
| | | | | Kind. | Time. | |
| no. ago | April 16 ... | Amput. fem.—moist salicyle dressing. | Apr. 27 | Cured | May 23 | May 30 |
| cture, mo. ago | Feb. 26 ... | As above ... ... ... | June 11 | Died | July 4 | — |
| rs. ago | July 1 ... | Amput. of left thigh; transfusion—moist salicyle dressing. | July 16 | Died | Aug. 12 | — |
| no. ago | Sept. 21 ... | Amput. of right thigh—dry salicyle dressing. | Sept. 26 | Cured | Oct. 14 | Nov. 10 |
| em. 1, 73. | Sept. 1, 1873 | Amput. of left leg—moist salicyle dressing. | June 10, 1874. | Cured | Aug. 15 | Oct. 8 |
| o. ago | Dec. 15, 1873 | Amput. of left leg—moist salicyle dressing. | May 22 | Cured | June 2 | Aug. 5 |
| 4 ... | Aug. 4 ... | Amput. of right leg ... ... | Aug. 12 | Died | Dec. 30 | — |
| | | Amput. of right thigh—moist salicyle dressing. | Nov. 10 | | | |
| 23 ... | Aug. 23 ... | Amput. of both legs ... ... | Aug. 23 | Died | Nov. 30 | — |
| | | Amput. of right thigh—moist salicyle dressing. | Nov. 23 | | | |
| 20 ... | Sept. 20 ... | Amput. of left leg—moist salicyle dressing. | Sept. 20 | Cured | Nov. 26 | Mar. 5, 1875. |
| e days ). | May 21 ... | Amput. of right humerus—moist salicyle dressing. | July 7 | Cured | Aug. 14 | Aug. 24 |
| years ). | April 21 ... | Amput. of right forearm—moist salicyle dressing. | May 4 | Cured | June 9 | June 19 |
| 1874 | May 15 ... | Exarticulation of hand—moist salicyle dressing. | May 21 | Almost cured. | June 30 | June 30 |
| 1874 | June 20 ... | Resection of head of humerus and erasion of glenoid cavity—moist salicyle dressing. | June 22 | Cured, with exceptn. of a fistula. | End of July. | Feb. 1875, still under treatment. |

dressing; from 26th day, dry ditto, which was left on eight days, suppuration being moderate. No fever.

No. 10. Slow healing process. *Vid. ante*, No. 6.

No. 11. Healing process interrupted by patient's restlessness. *Vid. ante*, No. 2.

No. 12. Healing process interrupted by retention of blood and partial sloughing of flaps. *Vid. ante*, No. 4.

No. 13. Admitted with suppuration of the left shoulder-joint, of four months' standing. The shoulder-joint much swelled, tense; the arm, as far as the wrist, inflamed and œdematous. Posteriorly, in the fossa supraspinata, a fistula discharging thin pus and extending towards the joint. Arm admits of only slight passive movement, with friction in joint and great pain.

On June 22, opening of joint by long incision anteriorly. Much pus in and around

| Number. | | Ward. | | Name of Patient. | Age. | Occupation. | Disease or Injury. |
|---|---|---|---|---|---|---|---|
| Current Number | General Register | No. | Surgeon. | | | | |
| 14 | 465 | 5 & 17 | B. | P. Johann ... | 28 | Labourer ... | Pseudarthrosis of left humerus, aft compound fracture. |
| 15 | 1077 | 17 | B. | D. Carl ... | 18 | Carpenter... | Compound fracture of left humer |
| 16 | 1234 | 4 | H. | Fr. Gottlob ... | 52 | Labourer ... | Compound fracture of olecranon |
| 17 | 1194 | 4 | Cr. | H. Heinrich ... | 32 | Cigar-maker | Necrosis of left ulna ... |
| 18 | 1187 | 3 | K. | Kr. Ernst ... | 45 | Labourer ... | Caries of joint of right hand. Fu gous suppuration of tendons left hand. |

the joint. Head of bone resected, fungous synovial membrane extirpated, glenoid cavity scraped. Disinfection of cavity with salicyle water. Two drainage-tubes, one of which, in the upper angle of the wound, passes through the fistula in the fossa supraspinata; the other, in the lower angle, passes out posteriorly through an artificial opening made for the purpose. The other parts of the wound united by deep sutures. Salicyle-wadding dressing, irrigation.

The healing process went on without swelling, *i.e.* the swelling present before the operation soon went down without pain or fever. The secretions from the wound always remained free from smell. The drainage-tubes were removed in the fourth week; the arm admits of good passive movement without pain. Slight erysipelas of three days' duration occurred during the healing process. The fistulous opening not quite closed in February, 1875, although no diseased bone could be detected.

No. 14. Eighteen weeks before admission, had compound fracture in the middle of the left upper arm, which is moveable, with a suppurating fistula and small sequestrum. This was removed February 25, 1874, the fibrous connection of ends of bone torn subcutaneously, and splints applied. No reaction worthy of mention, but also no reunion of fracture. On May 7, resection of ends of bone, with oblique surfaces held in position by strong silver wire. Moist salicyle dressing applied before removal of Esmarch's bloodless apparatus. Arm-board, salicyle irrigation. Dressing changed on second and fourth day. On latter, patient complained of severe pain; neighbourhood of wound red, but not swollen; copious rancid-smelling pus; protective blackish; temp. 120°. External wound re-opened in part. From sixth day no more fever; pus retains its bad smell; no swelling. On nineteenth day, drainage-tubes removed. From twenty-first day, dry dressing, the constant irrigation having produced a very troublesome miliary eruption extending from arm to back. From seventh week, dressing with simple ointment. The granulations contracted gradually, and by the aid of repeated capsular dressing the bones became more firmly united, and the suppuration decreased. Discharged August 18, with slight suppurating fistula. Point of resection tolerably firm. Arm in perforated starch dressing. Re-admitted in November following, enlargement of still existing fistular opening, and removal of small sequestrum and of silver wire. December 12, fracture almost healed. In January firm.

No. 15. Had on July 4, from a fall, an oblique fracture of the upper arm immediately

| ımmence-ment of isease, or Date of Injury. | Day of Admission. | Operation and Mode of Dressing. | Day of the Operation | Result. | | Date of Discharge |
|---|---|---|---|---|---|---|
| | | | | Kind. | Time. | |
| ıcture, | Feb. 23. ... | Resection, metallic sutures; | May 7 | Went out with fistula. | Aug. 18 | Aug. 18 |
| mo. ago | Re-admitted in Nov. | extraction of sutures and necrotic fragments of bone —moist salicyle dressing. | Nov. 25 | Cured | Jan. '75. | Jan. '75 |
| ly 4 ... | July 4 ... | Partial resection of elbow-joint | July 4 | Cured | End of Feb., 1875. | — |
| | | Sequestrum removed ... ... | Dec. 11 | | | |
| | | Do. do. —moist salicyle dressing. | Feb., 1875. | | | |
| g. 5 ... | Aug. 5 ... | Complete resection of elbow-joint—moist salicyle dressing. | Aug. 14 | Nov. 14 | Jan. 23, 1875. | — |
| 1 years go. | July 29 ... | Sequestrotomy—moist salicyle dressing. | July 29 | Almost cured. | Sept. 23 | Sept. 23 |
| ) years go. | July 28 ... | Resection of joint of right hand, incisions in left hand, and extirpation of fungus. | July 30 | | | |
| | | Amputation of right forearm —moist salicyle dressing. | Feb. 15, 1875. | Died ... | Feb. 16, 1875. | — |

above the elbow-joint, with a penetrating crushed wound at the olecranon. On enlarging the opening, the lower fragment of bone was found to be fractured also longitudinally, so that it fell into two almost equal pieces. These were removed, the wound disinfected with chloride of zinc, sewn up, drained at the upper angle, and provided with a closely attached, moist salicyle dressing, before the removal of Esmarch's bloodless apparatus. It was then bent at a right angle, fixed upon an arm-board, and irrigated with salicyle water.

The dressing had to be cut open after some hours on account of pain from distention, and as it was rather largely saturated with blood, it was renewed, by way of precaution, at the end of the second day. Considerable swelling of the fore- and upper-arm set in; wound secretions copious, without smell. Until fourth day, temp. 101.5°; from that date no fever. At the end of the fourth week, cavity of wound closed, but outer wound still granulating somewhat. A fistula remains. In December a sequestrum was removed, a second in February, 1875, but finally the cure became completed.

No. 16. Comminuted fracture. The patient was admitted a few hours after the injury with traumatic emphysema in the neighbourhood of the wound. Washing out with solution of carbolic acid, 1 : 20, of chloride of zinc, 1 : 8; moist salicyle dressing. In spite of this treatment, suppuration of joint, with temperatures up to 104°, which led to total resection on the ninth day. After the resection, also, a sub-febrile condition continued for a considerable time, and repeated incisions in the fore- and upper-arm were necessary on account of advancing suppuration. All the wounds closed, after three months, in November. In December, stiff œdema still exists of neighbourhood of resection and of forearm. Little active or passive motion of the joint placed at a right angle.

No. 17. Periostitic, fungous growths on the ulna, extending from the olecranon to the lower fourth. Has existed for ten years, with oscillations, without external opening. Fungus laid open by several incisions and scraped off; in olecranon, a granulating cavity of the size of a hazel-nut, opening posteriorly, from which a moveable sequestrum was taken. Drainage, moist salicyle dressing. Slow healing process without fever, except an increase of temperature to more than 102° in second week, without appreciable cause.

No. 18. Origin unknown. Patient very reduced from long confinement to bed and want of sufficient nourishment. Thickening of upper part of lungs and chronic bronchitis. On right side, hard, uniform swelling of the fingers, hand, and forearm, with

| Number. | | Ward. | | Name of Patient. | Age. | Occupation. | Disease or Injury. |
|---|---|---|---|---|---|---|---|
| Current Number. | General Register | No. | Surgeon. | | | | |
| 19 | 882 | 5 & 17 | B. | J. Friedrich ... | — | ... ... | Compound fracture of right leg ... |
| 20 | 1330 | 17 | B. | Sch. Wilhelm... | 20 | Labourer ... | Compound malleolar fracture of leg |
| 21 | 1420 | 3 | K. | B. Hermann ... | 20 | ... ... | Compound fracture of right leg ... |
| | | | | | | | GREATER AMPUTATIONS, RESECTION: |
| 22 | 635 | 4 | Cr. | F. Gottfried ... | 39 | Coachman... | Compd. fracture of left humerus compd. fract. of right metacarpus contused and lacerated wound of left thigh. |
| 23 | 1251 | 3 | K. | Str. Eduard ... | 24 | Brake-man | Compd. fracture of forearm .. |
| 24 | 285 | 5 | B. | F. Friedrich ... | 39 | Labourer ... | Pseudarthrosis of humerus ... ... |

numerous fistular openings leading to the carious joint. On left side, open, fungous growths, especially in the palm of the hand. On right side, resection of radius and ulna, extirpation of first row of the carpus, scraping off of softened portions of second row, besides numerous incisions in both hands and forearm, and extirpation of fungous granulations. Moist salicyle dressing, changed from sixth week for dry ditto. Strength increased, without fever, until September 24. From that time, advancing suppuration on left side and fever. Fresh incisions. In middle of October, slight erysipelas. Patient's condition became stationary in November, and will probably lead to amputation of the right forearm.

Amputation, February 15, 1875. Death on following day from exhaustion. Post-mortem showed advanced tuberculosis, and tubercles in bony tissue of carpus, and spongy substance of the radius.

No. 19. Run over by a loaded waggon. Extensive contusion of soft parts and separation of periosteum at point of fracture. Temp. 100.5°. Disinfection of wound with carbolic acid and a solution of chloride of zinc, 1 : 8, v. Brun's splint apparatus and Bardeleben's carbolic acid irrigation, which was changed in eight days for salicyle water on account of erosion of the skin. Course not favourable. In first eight days, great general swelling of leg, which led to several abscesses. Large portions of the contused skin sloughed. In first week, temp. up to 103°, and later, when fresh abscesses formed, as in July, high temperatures. From July 18, Heister's splint. On July 23, the loosened, necrotic ends of fractured bone could be removed. In August and September, no fever. September 17, fracture well united. Amount of shortening, 1, 2 inches.

No. 20. Fracture of ankle from a jump. Slight perforation of skin by upper broken end of fibula. Emphysema of skin up to knee. v. Brun's splint apparatus. Moist salicyle dressing.

First week, sub-febrile temperature and little suppuration. Second week, fever. Retained pus let off by incision. September 20—30, severe erysipelas extending to middle of thigh. From October 1, no fever. Consolidation of fracture early in November.

No. 21. Direct oblique fracture in middle of bone from fall of a heavy barrel upon it. Small skin wound at point of fracture of shin bone surrounded by emphysema. Extensive infiltration with blood. Heister's splint. Enlargement of small perforated wound to 0.8 inch. No disinfection of wound, moist salicyle dressing with irrigation.

During the two first weeks, numerous blood clots, free from smell, came from the

| Commencement of Disease, or Date of Injury. | Day of Admission. | Operation and Mode of Dressing. | Day of the Operation | Result. | | Date of Discharge |
|---|---|---|---|---|---|---|
| | | | | Kind. | Time. | |
| May 22 ... | May 23 ... | Sequestrotomy—moist salicyle dressing. | July 13 | Cured | Oct. 10 | Oct. 24 |
| Aug. 29 ... | Aug. 29 ... | Moist salicyle dressing ... | — | Cured | Early in Nov. | Feb. 3, 1875. |
| Sept. 19 ... | Sept. 19 ... | Moist salicyle dressing ... | — | Cured | Early in Nov. | Jan. 8, 1875. |
| **COMPOUND FRACTURES.—CARBOLIC DRESSING.** | | | | | | |
| April 1 ... | April 1 ... | Amput. of left humerus—Lister's gauze dressing, with boracic acid dressing. | Apr. 1 | Died ... | May 1 | — |
| Aug. 9 ... | Aug. 9 ... | Amput. of forearm—Lister's gauze dressing. | Aug. 9 | Cured | Sept. 9 | Oct. 7 |
| Jan. 22 ... | Jan. 23 ... | Resection, metall. sutures—Lister's gauze dressing. | Mar. 21 | Cured | July 15 | July 27 |

wound with little suppuration and gradual diminution of swelling. No fever after second day.

No. 22. Was run over and admitted a few hours after.

1. The compound fracture of the fourth and fifth metacarpal bones of right hand healed in a week with Lister's dressing, with the exception of a small point.

2. A deep, lacerated, contused wound of left thigh was treated by the open method, and healed well until the eleventh day. In the third week pus became offensive in smell.

3. The compound fracture of the left arm made immediate amputation necessary, which, although performed high up, still fell within the traumatic extravasation. Dressing strictly after Lister's method, with introduction of wound-wadding impregnated with boracic acid into it. Healing of the wound except at points of drainage. Fever during first ten days, morning temp. up to 102°, evening temp. up to 103.5°. From tenth day, no fever in the morning, evening temp. up to 102°. On eighteenth day, first rigor, temp. 104.5°. From that time the suppuration of the stump and of the wound in the thigh gradually became offensive, the united points gave way, embolic pyæmia ran its usual course, and, after four more exacerbations and icterus, caused death on May 1, twelve days after the first rigor. The post-mortem showed in the stump ill-smelling, suppurative osteo-myelitis as far as the spongy portion of the head of the humerus, but no pus in the large veins of the stump. Wound on thigh filled with offensive pus, no pus in veins of thigh. No abscesses in lungs. Abscesses in liver, with suppurative inflammation of neighbouring diaphragmatic pleura.

No. 23. Crushed by railway carriage. Comminuted fracture of the bones and laceration of the soft parts. Lister's gauze dressing of amputation-wound. No fever. Well healed on thirtieth day.

No. 24. Patient had been treated in 1873 for a fracture of the upper arm and discharged on July 5 as cured. Since then he had used the arm for hard manual labour. On January 2, 1874, while lifting a pail of water, he said, the arm broke at the old place. Ends of fracture much swelled and hard to the touch, considerable effusion of blood, no crepitation, no pain on motion. After several fruitless attempts at cure, the point of fracture was laid open on March 21, the fractured ends of bone scraped obliquely. Between them was found a small, smooth-walled cavity filled with serum. Silver wire suture for bones, suture for wound, drainage, Lister's gauze dressing, arm-board. No fever. Regular but very slow healing process. Good consolidation in sixteenth week.

| Number. | | Ward. | | Name of Patient. | Age. | Occupation. | Disease or Injury. |
|---|---|---|---|---|---|---|---|
| Current Number. | General Register | No. | Surgeon. | | | | |
| 25 | 861 | 4 | Cr. | E. Oscar ... | 14 | Painter's apprentice. | Comp. fracture of right humerus |
| 26 | 749 | 1 | K. | T. Hermann ... | 8 | Labourer's son. | Old luxation of right forearm |
| 27 | 759 | 2 | B. | Tr. Anna ... | 30 | Barmaid ... | Old luxation of left forearm |
| 28 | 871 | 3 | K. | H. Karl ... | 57 | Labourer ... | Caries of wrist-joint... ... |
| 29 | 772 | 3 | K. | B. Johann ... | 22 | Railway labourer. | Compd. fracture of left thigh |
| 30 | 1190 | 5 | B. | W. Johann ... | — | Labourer ... | Compd. fracture of cranium |
| | | | | SLIGHTER AMPUTATIONS, RESECTIONS, COMPOU | | | |
| 31 | 799 | 5 | B. | W. Max ... | 19 | Locksmith | Lacerated and contused wound forearm. |
| 32 | 783 | 3 | K. | B. Paul ... | 17 | Errand boy | Lacerated wound of forearm |
| 33 | 1252 | 4 | Cr. | W. Carl ... | 24 | Smith ... | Lacerated wound of forearm |

No. 25. Comminuted fracture in lower third of humerus from a fall. Wound above the olecranon. Extensive detachment of periosteum. Removal of loose splinters, disinfection of wound with chloride of zinc solution, 1 : 8, drainage, arm-board, Lister's gauze dressing. No fever. Drainage-tube omitted on fourteenth day. In the ninth week, a sequestrum of surface of upper end of bone, nearly three inches in length, was removed. In the fifteenth week, by aid of a capsular bandage, fracture well consolidated. Elbow-joint somewhat moveable.

No. 26. Long standing luxation of both bones of forearm backwards. Joint almost fixed in the half-bent position. Fruitless attempts at reduction. Resection of joint-ends of radius and ulna (two lateral incisions), drainage, rectangular arm-board, Lister's gauze dressing. No fever. Moderate suppuration, lasting five weeks. Good active motion.

No. 27. Luxation, operation and course as in foregoing case. The resected joint-head of the radius showed fibrous change of the cartilage and filling up of the articular cavity.

No. 28. Result of fungous ulceration of the joint. Carpal bones of first row almost entirely extirpated, resection of fungous joint-ends of radius and ulna, Lister's gauze dressing. No fever. Moderate suppuration. Healing with synosteosis. Good motion of thumb, very little of fingers. Discharged in the sixth week.

No. 29. Knee crushed by railway buffer. Fracture of the knee-epiphysis of femoral bone. Contused skin- and muscle-wound in popliteal space, reaching as far as the joint. Admitted second day after injury, with temp. of 102°. Leeches had been applied to the knee, and Scultet's dressing. Simple treatment of wound. Evening temp. under 102°. In the second week, synovial fluid oozed from a leech-bite in the neighbourhood of the patella, continuing about a fortnight. From sixteenth to thirty-first day, patient had very severe erysipelas, which spread from the leech-bite mentioned above downwards as far as the toes and upwards over the trunk, with the exception of the anterior surface of the chest. At the same time the wound in the popliteal space and in the joint were healing. In the fifth week, a large sub-cutaneous abscess formed above the patella, caused no doubt by the erysipelas, and a second deeper one corresponding to the internal

| mmence-ment of ease, or Date of Injury. | Day of Admission. | Operation and Mode of Dressing. | Day of the Operation. | Result. | | Date of Discharge |
|---|---|---|---|---|---|---|
| | | | | Kind. | Time. | |
| ay 19 ... | May 19 ... | Partial resection of elbow—Lister's gauze dressing. | July 21 | Almost cured. | Sept. 16 | Sept. 16 |
| arly in Feb. | April 25 ... | Resection of radius and ulna—Lister's gauze dressing. | Apr. 28 | Cured | Early in June. | July 5 |
| no. ago | April 28 ... | As above ... ... ... | May 1 | Cured | June 15 | June 30 |
| weeks ago. | May 21 ... | Resection of wrist-joint—Lister's gauze dressing. | May 23 | Cured | June 15 | July 6 |
| ril 18... | April 30 ... | Antiseptic treatment in second half of time. | — | Cured | Early in Sept. | Under treatment in Feby., 1875. |
| ly 28 ... | July 28 ... | Lister's gauze dressing ... | — | Almost cured. | Aug. 24 | Aug. 24 |
| ACTURES, AND OTHER INJURIES.—SALICYLE DRESSING. | | | | | | |
| ay 6 ... | May 6 ... | Moist salicyle dressing ... | — | Cured | June 5 | June 5 |
| ril 4 ... | April 4 ... | Do. do. ... | — | Cured | Apr. 23 | Apr. 23 |
| days ago. | Aug. 9 ... | Do. do. ... | — | Cured | Oct. 15 | Nov. 13 |

condyle of the femur. Both opened, drained, and dressed by Lister's method. During the next fortnight, evening temp. up to 102°. After the healing of both abscesses, a third was opened on the fortieth day under the vastus externus. Through the opening both condyles of the femur could be felt rough and deprived of cartilage posteriorly. Suppuration gradually ceased, with a sub-febrile temperature. From 123rd day simple dressing. On 131st day, the wound was closed. Synosteosis of the not perfectly extended knee-joint, patella adherent, thigh rotated somewhat outwards. Patient made a good recovery.

No. 30. From fall of a heavy stone from a height of about eight feet, slight symptoms of concussion, a large contused wound on right parietal bone; the exposed bone presented a longitudinal fracture about 1, 2 inches in length, and a short oblique one, one edge somewhat depressed.

Disinfection with chloride of zinc and carbolic acid; wound left partly open; Lister's gauze dressing. No fever, no necrosis. Cured in the fourth week.

No. 31. Lacerated and contused wound of skin and fascia, with exposure of crushed muscles. Loss of skin on forearm of the size of a small plate. Uninterrupted recovery with salicyle dressing renewed about every three days, and moistened occasionally with salicyle water; arm-board, rest, &c.

No. 32. Wound from broken glass of the skin, fascia and muscles of the forearm, 3, 2 inches long. Uninterrupted cure with salicyle paste in three weeks, with the exception of a small granulating spot.

No. 33. Wound from broken glass on inner surface of forearm, 6 inches long, obliquely above the wrist-joint. Had been neglected and was suppurating badly at time of admission. Gradual cicatrisation on attaching skin and sheaths of tendons to the tendons, especially those of the flexors of the fourth and fifth finger. Cicatrix at some points very painful when touched, as if some pieces of broken glass had been left in the wound. On cutting out the whole cicatrix, however, nothing was found. The edges of the wound over the exposed tendon of the flex. carpi uln. left by the excision were united by suture. Union by the first intention took place to a great extent with moist salicyle dressing. No fever. Good movement of hand and fingers when sent out.

| Number. | | Ward. | | Name of Patient. | Age. | Occupation. | Disease or Injury. |
|---|---|---|---|---|---|---|---|
| Current Number. | General Register | No. | Surgeon. | | | | |
| 34 | 1421 | 17 | B. | Gr. Carl ... | 61 | Locksmith | Compd. fracture of fingers ... |
| 35 | 1333 | 4 | Cr. H. | B. Carl ... | 20 | Labourer ... | Do. do. ... |
| 36 | 1198 | 4 | H. Cr. | O. Ludwig ... | 24 | Carpenter... | Do. do. ... |
| 37 | 1318 | 4 | Cr. H. | C. Gottlob ... | 35 | Do. ... | Do. do. ... |
| 38 | 1262 | 17 | H. | F. Carl ... | 38 | Labourer ... | Necrosis of great toe and 1st metatarsal bone. |
| 39 | 1321 | 4 | H. | G. Anton ... | 16 | Labourer ... | Compd. fracture of great toe |
| 40 | 1326 | 3 | K. | G. Andreas ... | 30 | Shunter ... | Compd. fracture of toes ... |
| | | | | SLIGHTER AMPUTATIONS, RESECTIONS, COMPOU | | | |
| 41 | 735 | 4 | Cr. | J. Johann ... | 59 | Builder ... | Incised and lacerated wound head. |
| 42 | 756 | 3 | K. | K. Carl ... | 23 | Porter ... | Contused wound of occiput |
| 43 | 761 | 3 | K. | G. Emil ... | 19 | Messenger | Do. do. |

No. 34. Injury by machinery. Compound comminuted fracture of fourth and fifth fingers, with laceration of the tendons and opening of the joint. Exarticulation of both fingers. Washing out with salicyle water, sutures. Moist salicyle dressing. Salicyle irrigation. No fever. Healing process retarded by partial sloughing of the skin of the flaps, and slight exfoliation of the extensor tendon of the fourth finger. Everything healed on November 2.

No. 35. Compound comminuted fracture of the third and fourth fingers, with laceration of the tendons and opening of the joints. Exarticulation of the third and fourth finger. Sutures, moist salicyle dressing. Sub-febrile up to third day. Temp. on two occasions 101.5°. No fever from fourth day. Some sloughing of flaps. Healing process otherwise uninterrupted. Wound closed on sixtieth day.

No. 36. Injury by a circular saw. Comminuted fracture of the second, fourth, and fifth fingers, deep lacerated wound of palm and second finger. Exarticulation of second and fourth fingers. Catgut sutures for all the wounds. Disinfection with solution of chloride of zinc 1 : 8. Moist salicyle dressing, arm-board, etc. Salicyle irrigation. No fever. Uninterrupted healing process. Cured on thirtieth day, with exception of some small granulating points.

No. 37. Injury of the palm of the hand by a circular saw, laceration of the soft parts of the ball of the thumb, and flexor tendons of index finger, comminuted fracture of second bone of metacarpus. Extirpation of index finger and peripheral fractured portion of second metacarpal bone. Sutures. Drainage. No fever. Uninterrupted healing process.

No. 38. Extensive necrosis of both bones of great toe and of the first metatarsal bone, with fistulous opening on under surface. Of traumatic origin and of a year's standing. Extirpation of the great toe with its metatarsal bone. Washing out with salicylic acid, sutures, drainage. Moist salicyle dressing with irrigation; healing process interrupted and retarded by the coming away of the tendon of the peroneus longus, which led to an abscess beneath the external ankle. Incision on fifth day. Sub-febrile course. On seven-

| mmence-ent of sease, or Date of Injury. | Day of Admission. | Operation and Mode of Dressing. | Day of the Operation | Result. | | Date of Discharge |
|---|---|---|---|---|---|---|
| | | | | Kind. | Time. | |
| t. 18 ... | Sept. 18 ... | Exarticulation of fingers iv. and v.—moist salicyle dressing. | — | Cured | Nov. 2 | Jan. 7, 1875. |
| g. 30 ... | Aug. 30 ... | Exarticulation of fingers iii. and iv.—same dressing. | — | Cured | Oct. 30 | Nov. 11 |
| y 29 ... | July 29 ... | Exarticulation of fingers ii. and iv.—same dressing. | — | Cured | Aug. 29 | Aug. 29 |
| g. 26 ... | Aug. 26 ... | Amput. of finger and 2nd metacarpal bone—same dressing. | — | Cured | Oct. 12 | Nov. 5 |
| r. ago. | Aug. 11 ... | Exarticulation of 1st metatarsal bone—same dressing. | — | Almost cured. | Nov. 11 | Nov. 11 |
| g. 27 ... | Aug. 27 ... | Exarticulation of great toe—moist salicyle dressing. | — | Cured | Sept. 29 | Oct. 17 |
| g. 29 ... | Aug. 29 ... | Exarticulation of toes i., ii., iii. and iv. | — | Cured | End of Nov. | — |

ACTURES, AND OTHER INJURIES.—CARBOLIC DRESSING.

| | | | | | | |
|---|---|---|---|---|---|---|
| r. 24 ... | Apr. 24 ... | Lister's gauze dressing ... | — | Cured | May 15 | May 15 |
| r. 27 ... | Apr. 27 ... | Do. do. ... | — | Cured | May 9 | June 23 |
| r. 28 ... | Apr. 28 ... | Do. do. ... | — | Cured | May 8 | June 19 |

teenth day, opening of an abscess on the outer side and upper third of the leg. Abscesses healed on October 19. Discharged November 11, with fistulous suppuration at point of amputation.

Pus probably entered during the operation into the sheath of the tendon of the peroneus longus and caused the separation.

No. 39. Compound comminuted fracture of the great toe from fall of a beam. Exarticulation, sutures, drainage, moist salicyle dressing. Healing process retarded by partial sloughing of flaps, otherwise uninterrupted. No fever. Wound closed on September 29.

No. 40. Compound fracture of first four toes from being run over (railway injury). Exarticulation of the injured toes. Moist salicyle dressing. Healing process retarded by sloughing of the upper flap. From end of September, dry salicyle dressing. Healed with thin cicatrix at end of third month.

No. 41. Wounds with an axe, four of which partially penetrated to the bone and splintered it. Loss of skin to the extent nearly of a crown piece.

Washing out with carbolic acid, 1 : 20; catgut sutures, gauze dressing. Healing by the first intention, except at point of loss of skin. Closure of the latter on the twenty-first day. No fever.

No. 42. Large, contused flap wound of occiput, with exposure of bone, 4 inches in extent, from fall of a piece of iron weighing one hundredweight. Slight concussion of brain. Washing out of the wound with carbolic acid, 1 : 5, Lister's gauze dressing. Dressing renewed for first time on seventh day, for second time on twelfth day. Wound healed. No fever.

No. 43. Large, contused flap wound of lower part of occiput from falling of a barrel. The flap can be turned back over the ear, the bone is laid bare to the extent of a crown piece. Washing out with carbolic acid 1 : 20, sutures, drainage behind the ear, Lister's gauze dressing. On fourth day, first change of dressing, wound healed as far as the point of drainage; on tenth day this also. No fever.

| Number. | | Ward. | | Name of Patient. | Age. | Occupation. | Disease or Injury. |
|---|---|---|---|---|---|---|---|
| Current Number. | General Register | No. | Surgeon. | | | | |
| 44 | 880 | 3 | K. | N. Carl ... | 19 | Law student | Contused wound of forehead; la rated wound of hand. |
| 45 | 935 | 3 | K. | Sch. Carl ... | 21 | Miller ... | Contused wound of occiput |
| 46 | 1015 | 2 | B. | K. Johanne ... | 49 | Servant ... | Lacerated wounds of head and fa |
| 47 | 1096 | 5 | B. | B. Gustav ... | 21 | Shunter ... | Contused wound of forehead |
| 48 | 649 | 5 | B. | S. Carl ... | 28 | Locksmith | Wound of neck and head ... |
| 49 | 722 | 3 | K. | S. Christian ... | 18 | Shoemaker | Wound of forearm ... ... |
| 50 | 873 | 4 | Cr. | U. Christian ... | 46 | Coffee-house keeper. | Do. do. ... ... |
| 51 | 907 | 5 | B. | C. Ernst ... | 22 | Cartwright | Do. do. ... ... |
| 52 | 1019 | 4 | Cr. H. | K. Emil ... | 17 | Bookseller's apprentice. | Wound of left elbow ... |
| 53 | 683 | 5 | B. | G. Franz ... | 18 | Locksmith | Anchylosis of finger ... |
| 54 | 933 | 4 | Cr. | N. Ernst ... | 22 | Labourer ... | Compd. fracture of right index fin |
| 55 | 1060 | 4 | Cr. | P. Friedrich ... | 30 | Labourer ... | Contused wound of palm ... |

No. 44. Fall from a railway carriage in motion. Contused, gaping wound of left side of forehead two inches long. Bone laid bare to the extent of a crown piece. Wound filled with sand and dirt. Lacerated wound of the fingers, with laying open of the sheaths of the flexor tendons of third, fourth, and fifth fingers, in the middle finger with laceration of flexor tendons and laying open of second finger-joint. Painting with solution of carbolic acid 1:5. Careful union of wound. Wound on head firmly healed May 6 (thirteenth day), with two dressings only. Finger-wound closed on twenty-first day. No fever.

No. 45. Injury from a fall. Contused flap-wound more than six inches long on occiput, oblique with convex edge above, with considerable exposure of bone on both sides. Washing out with carbolic acid 1:20, sutures, gauze dressing. Wound healed on June 15. Dressed twice only. No fever.

No. 46. Fall of a window-sash from second floor on head. Concussion of brain slight. Wound of scalp 2.2 inches long. Wound of forehead 2 inches long with exposure of bone. Wound at root of nose 0.8 inches long, much contused down to the bone. Upper lip split open by a jagged, penetrating wound. In most of the wounds, bits of glass, in all, sand and dirt. Washing out of the wound with carbolic acid 1:10. Sutures, Lister's gauze dressing for occiput and forehead. When the dressing was taken off on the eighth day, the head-wounds were healed. No fever.

No. 47. Fall on to pavement from height of eight feet. Contused, burrowing wound of forehead 2.4 inches long extending to the pericranium. Washing out with carbolic acid 1:20. Sutures. Lister's gauze dressing. Healing by the first intention except at a small point which corresponded to a defect arising from the sloughing of the edge of the wound. On the tenth day, this also was healed. No fever.

No. 48. Wound with a knife beneath the angle of the jaw. Course of the wound traceable with the finger as far as the transverse process of one of the cervical vertebræ.

| mmence-ent of ease, or ate of njury. | Day of Admission. | Operation and Mode of Dressing. | Day of the Operation. | Result. | | Date of Discharge |
|---|---|---|---|---|---|---|
| | | | | Kind. | Time. | |
| y 23... | May 23 ... | Lister's gauze dressing ... | — | Cured | June 14 | June 14 |
| ie 3... | June 3 ... | Do. do. ... | — | Cured | June 15 | June 26 |
| ie 22... | June 22 ... | Do. do. ... | — | Cured | July 18 | July 18 |
| y 6... | July 6 ... | Do. do. ... | — | Cured | July 16 | July 16 |
| . 4 .. | Apr. 4 ... | Do. do. ... | — | Cured | May 8 | May 16 |
| . 21... | Apr. 21 ... | Do. do. ... | — | Cured | May 28 | June 1 |
| y 21... | May 21 ... | Do. do. ... | — | Cured | May 26 | May 26 |
| y 29... | May 29 ... | Do. do. ... | — | Cured | June 4 | June 23 |
| e 22... | June 22 ... | Cautery with chloride of zinc —same dressing. | — | Cured | July 18 | Aug. 1 |
| ? ... | May 16 ... | Exarticulation of v. finger—same dressing. | — | Cured | May 20 | May 27 |
| e 3... | June 3 ... | Disinfection with chloride of zinc, 1 : 8—same dressing. | — | Cured | July 8 | July 13 |
| e 30... | June 30 ... | Same disinfection and dressing | — | Cured | July 17 | July 29 |

Moderate hæmorrhage. Washing out with carbolic acid 1 : 20. Drainage to bottom of wound. Lister's gauze dressing, changed once or twice daily during the first eight days; at first sub-febrile course, on eighth day free from fever. Drainage tube removed on fifteenth day. Wound healed on thirty-first day.

No. 49. Stab with knife through skin and fascia. Washing out with carbolic acid 1 : 10, Lister's gauze dressing. Suppuration. No fever. Cured in five weeks.

No. 50. Wound from broken glass. Radial artery divided. Tendon of flexor carp. rad. partly cut. Catgut ligature. Washing out with carbolic acid 1 : 20. Sutures. Union by first intention.

No. 51. Wound with a chisel. Branch of radial artery and tendon of flexor carp. rad. divided. Catgut ligature, Washing out with carbolic acid 1 : 20. Sutures. Union by first intention.

No. 52. Contused wound from being driven over. Skin and fascia of elbow torn, biceps laid bare as far as the middle. Washing out with solution of chloride of zinc 1 : 8. Wound of skin pared and sewn up. Drainage. Lister's gauze dressing. No fever. Cured on twenty-sixth day.

No. 53. Exarticulation of fifth finger for anchylosis of joints of the phalanx. Flap formed on dorsum. Catgut ligatures. Sutures. Lister's gauze dressing. Wound closed on fourth day. Discharged on tenth day with firm, linear cicatrix.

No. 54. Compound comminuted fracture of first phalanx of index-finger. Removal of loose splinters. Washing out with chloride of zinc. Lister's gauze dressing. Wound closed on twenty-seventh day. Fracture firmly united on twenty-fifth day. Good movement of finger. No fever.

No. 55. Contused wound of skin and fascia of the palm of the hand. Washing out with solution of chloride of zinc 1 : 8. Edges of wound pared and sewn up. Lister's gauze dressing. No fever. Cured on seventeenth day.

| Number. | | Ward. | | Name of Patient. | Age. | Occupation. | Disease or Injury. |
|---|---|---|---|---|---|---|---|
| Current Number. | General Register | No. | Surgeon. | | | | |
| 56 | 1216 | 1 | K. | D. Emilie ... | 2 | Labourer's child. | Sarcoma of third finger ... |
| 57 | 1240 | 2 | B. | Sch. Amalie ... | 37 | Shoemaker's wife. | Neuralgia of little finger ... |
| 58 | 1283 | 13 | H. | W. Ernst ... | 18 | Smith ... | Contused wound of middle finger |
| 59 | 1292 | 4 | Cr. | Sch. Friedrich | — | House porter | Contused wound of finger ... |
| 60 | 1302 | 3 | K. | R. Friedrich ... | 21 | Labourer ... | Contused wound of leg ... |
| | | | | | | | ABSCESSES AND OTHER SUPPU |
| 61 | 1201 | 135 | H. | Sch. Theresa ... | 20 | Prostitute... | Subfacial abscess of infrasp region of scapula. |
| 62 | 860 | 5 | B. | S. Robert ... | 20 | Ironfounder | Suppurative, præpatellary bursiti |
| 63 | 1149 | 5 & 17 | B. | Th. Johann ... | 27 | Labourer ... | Suppurative, præpatellary bur and peribursitis. |
| 64 | 1112 | 3 | K. | G. Johann ... | 51 | Tailor ... | Acute suppuration of knee-joint |
| 65 | 1211 | 3 | K. | W. Conrad ... | 18 | Tinman ... | Empyema ... ... ... |
| | | | | | | | ABSCESSES AND OTHER SUPPU |
| 66 | 1332 | 2 | B. | Th. Dorothea... | 58 | Labourer's widow. | Deep abscess of neck ... |

No. 56. The exarticulation-wound, although sewn up and treated by Lister's gauze dressing, healed by granulation. No fever.

No. 57. The wound healed almost entirely by the first intention.

No. 58. Suppurating, contused wound of third phalanx with exposure of bone. Exarticulation of phalanx. Catgut ligature. Sutures. Healed on twenty-second, with the exception of a small spot.

No. 59. Injury by machinery. Exarticulation in second phalangeal joint. Catgut ligature. Sutures. Gauze dressing. Healed on eleventh day.

No. 60. From fall upon a blunt piece of iron, which penetrated through skin and fascia in neighbourhood of ligamentum patella. No escape of synovia. Lister's gauze dressing. Cured in three weeks. No fever.

No. 61. Admitted July 29 for chancre of genitals, has had from September 1—24 an attack of acute articular rheumatism without cardiac complication. From September 21, an inflammatory swelling on the posterior surface of the right shoulder-blade, with slight fever, which, when opened on September 26, discharged 600 cubic centimetres of pus. Drainage. Washing out with salicyle water, dry salicyle dressing. From third day no fever. On fourth day cavity of abscess closed, on eighteenth day drainage-canals also healed.

No. 62. Temperature on admission 102.5°. About 80 cubic centimetres of pus discharged, partly from the bursa, partly from the neighbouring cellular tissue. Washing out with salicyle water. Drainage and irrigation. No fever.

No. 63. Temperature on admission 103.5°. Suppuration extending to middle of thigh. About 500 cubic centimetres of pus mixed with blood and shreds of cellular tissue came away. Four drainage-tubes. Washing out with salicyle water. Salicyle dressing with irrigation. From second day free from fever. Healing process interrupted

| mence-nt of ase, or ate of jury. | Day of Admission. | Operation and Mode of Dressing. | Day of the Operation. | Result. | | Date of Discharge |
|---|---|---|---|---|---|---|
| | | | | Kind. | Time. | |
| 1... | Aug. 2 ... | Exarticulation of third finger —same dressing. | — | Cured | Aug. 12 | Aug. 12 |
| ng nding. | Aug. 6 ... | Exarticulation of little finger —same dressing. | Aug. 5 | Cured | Aug. 10 | Aug. 20 |
| 9... | Aug. 17 ... | Exarticulation of phalanx iii. —Lister's gauze dressing. | Aug. 18 | Cured | Sept. 10 | Sept. 21 |
| 19... | Aug. 19 ... | Exarticulation of finger—same dressing. | Aug. 19 | Cured | Sept. 1 | Sept. 5 |
| 22... | Aug. 22 ... | Exarticulation of toe—same dressing. | — | Cured | Sept. 14 | Sept. 14 |
| **IS.—SALICYLE DRESSING.** | | | | | | |
| — | — | Incisions, drainage—dry salicyle dressing. | Sept. 26 | Cured | Oct. 13 | Oct. 23 |
| 9... | May 18 ... | Incisions, drainage—moist salicyle dressing. | May 19 | Cured | June 10 | June 19 |
| 5... | July 19 ... | Incisions, drainage — same dressing. | July 20 | Cured | Sept. 29 | Oct. 3 |
| dle of y. | July 12 ... | Incisions, drainage — same dressing. | July 14 | Died | Aug. 10 | — |
| ,1874 | Sept. 7 ... | Incisions, drainage — same dressing; afterwards dry. | July 31 Oct. 7 | Cured | Feb. 28 | — |
| **IS.—CARBOLIC DRESSING.** | | | | | | |
| nonth . | Aug. 29 ... | Incisions, drainage — Lister's gauze dressing. | Aug. 30 | Cured | Sept. 16 | Sept. 16 |

on fourteenth day by erysipelas, which lasted a week. Sub-febrile course. Not entirely cured until seventieth day.

No. 64. Ill-fed subject. Healed necrosis of right tibia of 1855. Two months ago, inflammation of right knee-joint. Distinct fluctuating swelling as far as middle of thigh, very painful, red, temperature 102.2°. July 14, incisions, washing out with salicyle water, drainage, extension, moist salicyle dressing, irrigation. Until August 3, sub-febrile course, with occasional evening temperature up to 104°. Decreasing suppuration. On August 3, severe erysipelas set in which, starting from one of the drainage openings, spread over the lower part of the trunk, with evening temperature of 105°. Death from œdema of the lungs on the eighth day of the erysipelas.

No. 65. Commencement of the illness, April, 1874. From July 3 to September 7 in medical wards. Extensive pleuritic effusion on right side. At the end of July, abscess in second right intercostal space which, when punctured, discharged some bloody pus. From time to time copious, purulent expectoration, so that a communication between the bronchiæ and the empyema was to be suspected. On July 31, 1,000 cubic centimetres of pus were removed by puncture. On October 7, fresh puncture, with counter-puncture in back for drainage. Washing out with salicyle water repeated daily. The patient, free from fever, gradually recovered from the extreme exhaustion, and got up on October 20. The empyemic cavity still contained on November 7, 80 cubic centimetres. The patient was thoroughly restored in February, 1875; the upper half of the lung acted; the empyemic cavity was not closed, although the ribs were strongly drawn in and the liver drawn up. The patient wore a silver tube through which some pus flowed. Fistula healed end of February.

No. 66. Caused, perhaps, by a contusion four weeks ago. A fluctuating tumour on left side, extending from lower edge of jaw to clavicle. Temperature on admission 101.5°.

| Number. | | Ward. | | Name of Patient. | Age. | Occupation. | Disease or Injury. |
|---|---|---|---|---|---|---|---|
| Current Number. | General Register | No. | Surgeon. | | | | |
| 67 | 1221 | 4 | Cr. | U. Ernst ... | 64 | Weaver ... | Abscess of infra-maxillary region |
| 68 | 1222 | 2 | B. M. | F. Caroline ... | 61 | Nurse ... | Abscess of hand and forearm af paronychia. |
| 69 | 1208 | 13A | H. | B. Franz ... | 51 | Joiner ... | Sub-fascial abscess of thigh |
| 70 | 1064 | 5 | B. | H. Gustav ... | 15 | Apprentice | Abscess of thigh after paronychia |
| 71 | 865 | 56 | H. | M. Friedrich ... | 26 | Law student | Fungous ostitis of wrist ... |
| 72 | 599 | 2 | B. | Th. Anna ... | 14 | Servant ... | Abscess of thigh (periostitis) |
| 73 | 1095 | 5 | B. | K. Ernst ... | 27 | Mason ... | Abscess of thigh (periostitis) |
| 74 | 611 | 1 | K. | S. Arthur ... | 5 | Tradesman's son. | Coxitis. Abscess of thigh ... |
| 75 | 659 | 1 | K. | W. Elisabeth... | 14 | Tradesman's daughter. | Do. do. ... |

August 30, incision, drainage, Lister's gauze dressing. September 10, drainage-tube removed. September 16, incision-wound healed. No fever.

No. 67. Hot, fluctuating tumour formed in four weeks, which extended from lower edge of right jaw to first rib. Temperature on admission 99°. Incision, drainage, Lister's gauze dressing. Cured on fourteenth day.

No. 68. Paronychia of thumb of fourteen days' standing. Opened externally a week ago. Temp. on admission 103°. Swelling of hand and forearm. Deep fluctuation in palm of hand and flexor side of forearm developed itself in spite of rest, good position, and ice. On August 12, numerous incisions at the fluctuating points. Drainage, Lister's gauze dressing. No fever. On September 13, removal of drainage-tubes. On September 17, everything closed. When discharged, October 19, scarcely any movement of phalangeal joints, somewhat more in metacarpal joints. Wrist free.

No. 69. Patient has been emaciated for years. During eight weeks previous to admission, a fluctuating tumour formed on right thigh, extending on the outer side from the trochanter to the knee. Temp. on admission sub-febrile. Incisions, 1,500 cubic centimetres of pus, drainage, Lister's gauze dressing. Sub-febrile course until August 17. Drainage-tubes removed September 12. From October 3, dressing with simple ointment. At the end of October, points of incision not yet cicatrised. Fistulous suppuration still going on in February, 1875.

No. 70. Paronychia of right index finger of five days' standing, neglected. On day of admission, temp. 104°, with great inflammatory swelling and suppuration of index finger and back of the hand, then of the forearm. Incisions, Lister's gauze dressing. Decrease of fever. No fever from July 3—11. From July 12, swelling of the left thigh, deep and not sharply defined, with high temperatures. On July 30, opening of deep abscess, 1,200 grammes of pus, drainage, gauze dressing. August 10, cavity of abscess closed except at point of drainage. No fever after opening of abscess. Suppuration of hand and arm also at an end.

No. 71. For a year fluctuating tumour on back of hand as large as a pigeon's egg, corresponding to second metacarpal bone. Incision, sixty grammes of serous, flaky pus discharged. Periosteum of second metacarpal bone loosened. Carbolic acid spray, cauterisation with solution of chloride of zinc, 1 : 8, sutures leaving open corners of wound.

| nmence-ient of ease, or ate of njury. | Day of Admission. | Operation and Mode of Dressing. | Day of the Operation. | Result. | | Date of Discharge |
|---|---|---|---|---|---|---|
| | | | | Kind. | Time. | |
| d of me. | Aug. 3 ... | Same treatment—same dressing. | Aug. 3 | Cured | Aug. 17 | Aug. 17 |
| d of aly. | Aug. 3 ... | Incisions, drainage—Lister's gauze dressing. | Aug. 12 | Cured | Sept. 17 | Oct. 18 |
| dy in me. | Aug. 1 ... | Do. do. ... | Aug. 3 | Cured, with exceptn. of a fistula. | End of Oct. | Feb. 1875, still under treatment. |
| onychia days. | July 1 ... | Do. do. ... | July 30 | Cured | Aug. 10 | Sept. 15 |
| r. ago | May 19 ... | Do. do. ... | May 19 | Almost cured. | June 12 | June 12 |
| ht days o. | Mar. 23 ... | Do. do. ... | April 8 | Cured | May 5 | May 20 |
| onth o. | July 17 ... | Do. do. ... | July 18 | Uncured | — | Aug. 14 |
| rs. ago | Mar. 28 ... | Do. do. ... | June 17 | Imprvd. | — | Sept. 2 |
| years o | April 7 ... | Do. do. ... | April 8 | Imprvd. | — | June 4 |

Gauze dressing. No fever. Gradual cessation of suppuration. On July 16, still a small granulating spot.

No. 72. Temp. on admission, 104.5°, diffused painful swelling of lower third of right thigh, with slight effusion into knee-joint. After eight days of high fever, deep-seated fluctuation on inner surface of thigh. Therewith, decrease of temp. Abscess opened on April 8, 800 cubic centimetres of pus, free from smell but mixed with shreds of cellular tissue, was discharged. The bone could be felt bared to the extent of four inches. Carbolic acid spray. No washing out. Drainage. Lister's gauze dressing. Sub-febrile course, with gradual decrease of suppuration. Drainage-tubes removed on nineteenth day after opening of abscess. On twenty-seventh day everything closed. The femur at the point found bare, and for some distance beyond, felt much thickened. Patient began to walk without pain. Temperature normal, but pulse still 120 at time of discharge as during the fever.

No. 73. Fluctuating tumour larger than the fist, nearly bursting, on right thigh below the groin. Groin free, nothing observable in pelvis. Incisions on July 8, 900 grammes of pus discharged, drainage, Lister's gauze dressing. The discharge decreased in the first few days, after which it continued pretty stationary. From the middle of July, the patient complained of pain above the symphysis pubis and an induration painful on pressure could be felt there. This state of things continued, with temperatures occasionally as high as 102.2°. On August 17, he was discharged uncured at his own request.

No. 74. Had never been under regular treatment since beginning of his illness two years and a half ago. Large abscess beneath the groin exterior to the vessels of the thigh. Contraction of hip-joint at a right angle. Numerous indolent glandular swellings. Temp. 102°. Incisions on June 17, drainage, Lister's gauze dressing. Gradual decrease of discharge. No fever. Discharged September 3 with moderate suppuration. Treated afterwards as out-patient. Drainage-openings not closed in December, or in February, 1875.

No. 75. Treated two years ago for coxitis and apparently cured. Six months ago, fresh pain and swelling. At present a large abscess exterior to the vessels of the thigh. Incisions on April 8, drainage, Lister's gauze dressing. In the middle of May, cavity of abscess closed, but still slight suppuration at the granulating point of drainage. No fever

TUMOURS.—SA

| Number. | | Ward. | | Name of Patient. | Age. | Occupation. | Disease or Injury. |
|---|---|---|---|---|---|---|---|
| Current Number. | General Register | No. | Surgeon. | | | | |
| 76 | 1117 | 3 | K. | W. Carl ... | 23 | Stocking-maker. | Cystic struma ... ... |
| 77 | 862 | 3 | K. | E. Emil ... | 33 | Saddler ... | Recurrent sarcoma of chest |
| 78 | 794 | 2 | B. | B. Amalie ... | 50 | Weaver's wife. | Carcinoma of breast... ... |

TUMOURS.—C

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| 79 | 1215 | 2 | B. | Tr. Emma ... | 20 | Servant ... | Meliceris of forehead ... |
| 80 | 1466 | 17 | B. | D. Carl ... | 61 | Weaver ... | Meliceris of eyelid ... ... |
| 81 | 1183 | 2 | B. | K. Pauline ... | 20 | Servant ... | Prepatellary hygroma ... |
| 82 | 783 | 4 | Cr. | B. Julius ... | 17 | Labourer ... | Hydrocele ... ... ... |
| 83 | 920 | 5 | B. | W. Johann ... | 47 | Weaver ... | Double hydrocele ... ... |
| 84 | 913 | 4 | Cr. | J. Carl ... | 52 | Joiner ... | Hydrocele ... ... ... |

except during a slight attack of erysipelas which lasted eight days. Drainage-opening not yet closed in November.

No. 76. Large cystic tumour on right side, extending from the lobe of the ear to the sternum. Laid open in its whole length, it discharged brownish matter from an upper and a lower cavity. Wall of cyst thick, partly chalky. Walls of cyst cut off where projecting beyond the skin. Turning back of cut edge of cyst to check hæmorrhage. Double suture, superficial and deep, for union by the first intention, drainage, washing with salicyle water. Up to evening of nineteenth day temp. rose gradually to 104.5°. Discharge offensive. All sutures removed. Separation of superficial adhesion. Moist salicyle dressing. Gradual cleansing of wound with throwing off of chalky portions of walls of cyst. From twenty-third day, salicyle irrigation. From twenty-fifth day, severe erysipelas, which continued to spread until August 10 downwards over the whole trunk, with temperatures up to 104° and more. From August 10 to October 10 no fever. Gradual closure of cavity extending about two inches below upper edge of sternum.

No. 77. Brown mole on skin of right pectoralis major extirpated in 1871 because of cancerous degeneration (melanotic carcinoma). Remains of mole unchanged, but eight weeks after extirpation, return in axilla, which was filled with a large ulcerating tumour. On May 18, extirpation with extensive exposure of the large vessels, &c. Moist salicyle dressing of the large wound-surface. No fever from third day. Discharged July 17 with slight remains of granulating surface. In October, no relapse, no cicatricial strangulation of vessels in axilla. In November, three small fresh tumours in neighbourhood of nipple.

No. 78. Commencement observed two years ago as a nodule under the skin. At present, a hard, central tumour as large as an egg; skin free. Brownish serum flows guttatim from nipple. Tumours as big as a bean in the axilla. On May 7, extirpation of mamma. Extirpation of tumours in axilla deferred on account of collapse. Sutures, drainage, moist salicyle dressing. Sub-febrile course with strong nervous excitement.

YLE DRESSING.

| Commencement of Disease, or Date of Injury. | Day of Admission. | Operation and Mode of Dressing. | Day of the Operation | Result. | | Date of Discharge |
|---|---|---|---|---|---|---|
| | | | | Kind. | Time. | |
| Many yrs. ago. | July 13 ... | Incision, drainage—Moist salicyle dressing. | July 15 | Cured | Sept. 30 | Oct. 12 |
| Three yrs. ago. | May 19 ... | Extirpation — moist salicyle dressing. | May 20 Recurrent Nov.'74. | Cured | July 11 | July 11 |
| Two years ago. | May 5 ... | Do. do. ... | May 20 | Cured | May 25 | May 27 |

OLIC DRESSING.

| | | | | | | |
|---|---|---|---|---|---|---|
| ong time ago. | Aug. 1 ... | Excision—Lister's gauze dressing. | Aug. 1 | Cured | Aug. 6 | Aug. 6 |
| o. | Sept. 28 ... | Excision, drainage — Lister's gauze dressing. | Sept. 28 | Almost cured. | Sept. 30 | Sept. 30 |
| o. | July 25 ... | Incision, drainage — Lister's gauze dressing. | July 27 | Cured | Aug. 24 | Aug. 24 |
| bout a year ago. | April 4 ... | Incision, cauterisation, suture —Lister's gauze dressing. | May 8 | Cured | June 1 | June 6 |
| long time ago. | June 1 ... | Same treatment, with drainage —same dressing. | June 3 | Cured | June 27 | June 27 |
| o. | May 30 ... | Extirp. of tunic. vagin. and testicle—same dressing. | June 3 | Cured | June 26 | June 27 |

Good healing of wound. Above the wound an abscess, which was opened on the 14th and closed rapidly. A second above the first with similar course. Both abscesses contained thick pus. All the wounds were closed on May 27.

No. 79. Excision, sutures, no drainage. Lister's gauze dressing.

No. 80. Cyst the size of a plum dissected out. Sutures, lower corner of wound drained.

No. 81. Incision lets out yellow serum. Inner surface of cyst partly covered with tufts. Painting with concentrated carbolic acid, sutures, drainage of lower corner of wound. Lister's dressing closely applied. No immediate union. For fourteen days some sero-purulent discharge from drainage-opening. No fever. On twenty-fourth day, point of drainage not yet quite skinned over.

No. 82. Incision. Inner surface of soft, thin tunica vaginalis painted with a mixture of equal parts of alcohol and carbolic acid, except over testicle. Careful checking of hæmorrhage. Deep pearl-stitch suture with silver wire, to bring into contact the walls of the cyst. Superficial silk suture. No drainage. Lister's gauze dressing. Moderate swelling, no suppuration. No fever. Patient got up on tenth day and was discharged on twenty-eighth day. Tunica vaginalis can be felt as a hard cord.

No. 83. Had been tapped frequently on both sides. Incision on both sides. The rest as in the last case but with drainage from the lower corners of both wounds. During the first days, a bloody serous discharge. Moderate swelling, no fever. Drainage-tubes removed on third day. Points of drainage closed on thirteenth day. Discharged on twenty-fifth day. Tunica vaginalis on both sides can be felt as a hard cord.

No. 84. Had been tapped frequently. Weakly, ill-nourished subject. Sac opened to the extent of eight inches. Wall in places as thick as the thumb, testicle pressed flat. Removal of testicle and tunica vaginalis as far as inguinal ring. Catgut ligature of spermatic cord, removal of superabundant skin of scrotum, the remainder firmly united by deep pearl-stitch suture and superficial button-stitch suture. Drainage of upper and

| Number. | | Ward. | | Name of Patient. | Age. | Occupation. | Disease or Injury. |
|---|---|---|---|---|---|---|---|
| Current Number. | General Register | No. | Surgeon. | | | | |
| 85 | 1116 | 17 | B. | S. Bertrand ... | 24 | Shopman ... | Adenoma of testicle ... ... ... |
| 86 | 832 | 2 | B. | H. Christiana... | 47 | Labourer's wife. | Neuroma of the arm ... ... |
| 87 | 930 | 89 | Cr. | G. Christiane... | 55 | — | Carcinoma of breast and axillary glands. |
| 88 | 834 | 82 | B. | Sch. Sophie ... | 27 | — | Adenoma of breast ... ... ... |
| 89 | 1184 | 89 | K. | R. Emilie ... | 57 | — | Carcinoma of breast and axillary glands. |
| 90 | 1127 | 2 | B. | B. Amalie ... | 50 | Weaver's wife. | Carcinoma of axillary glands ... |
| | | | | SECOND DIVISION, FROM OCTOBER 1, 1874, TO JANUARY | | | |
| | | | | GREATER AMPUTATIONS, RESECTIONS. | | | |
| 91 | 1808 | 4 | C. | R. Friedrich ... | 45 | Clerk ... | Contused, lacerated wound of leg, etc. |
| 92 | 1538 | 2 | B. | Z. Helene ... | — | — | Compound fracture of tarsus and metatarsus. |
| 93 | 106 | 3 | K. | S. August ... | 18 | Labourer ... | Gangrene of both feet and legs from cold. |

lower corner of wound. Lister's dressing. Drainage-tubes removed on third day. Everything healed on eighth day except drainage-canals, which closed on twenty-third day. No fever and but little swelling.

No. 85. Soft, elastic, pear-shaped, nodular tumour as large as a fist. Operation and dressing as in last case (84). Moderate swelling, no fever. Healed on tenth day with exception of lower drainage-opening, which also healed on the sixteenth day. The growth proved to be soft adenoma with hæmorrhagic and yellow points of softening.

No. 86. Firm tumour above the internal condyle of the humerus, the size of a cherry, moveable under the skin, on pressure, very painful for some hours, as well as surrounding parts. Extraction, sutures, no drainage. Lister's gauze dressing. Healed on the 8th day, although the cavity of the wound became filled with blood on the 4th day.

The growth proved to be soft sarcoma, through which nerves could be seen with the naked eye running to the skin.

No. 87. Mamma occupied by firm small tumours; beneath the nipple, breach of surface of the size of a crown piece. Adhesion to pectoral muscle. Extirpation. The wound admitted of union by suture. Drainage, Lister's gauze dressing. Œdema of upper arm on side of operation, decreasing from 3rd to 14th day. No union by the first intention at the bottom of the wound. From the first, evening temperatures up to 104°. From the eighth day, painful swelling of the left knee-joint, which soon began to fluctuate. In the third week a bedsore from deep-seated extensive sloughing of the subcutaneous cellular tissue of the buttocks, which caused copious suppuration. From that time, the operation-wound also progressed less favourably. Separation of the healed surface; thin, serous pus. Death on thirty-ninth day from exhaustion.

Post-mortem. Operation-wound partly healed. Pus in left knee-joint. Great destruction of soft parts about the sacrum, small nodules of cancer in both lungs.

No. 88. Patient very weak and chlorotic. Tumours as large as hazel-nuts. Breast small. Extirpation of breast. Sutures, drainage, Lister's gauze dressing. Complete cure on eleventh day.

| Commencement of Disease, or Date of Injury. | Day of Admission. | Operation and Mode of Dressing. | Day of the Operation | Result. | | Date of Discharge |
|---|---|---|---|---|---|---|
| | | | | Kind. | Time. | |
| A year ago | July 13 ... | Extirpation—same dressing ... | July 14 | Cured | July 30 | July 31 |
| Thirteen years ago | May 11 ... | Do. do. ... | May 18 | Cured | May 26 | May 21 |
| Several yrs. ago. | June 2 ... | Extirpation—Lister's gauze dressing. | June 3 | Died | July 12 | — |
| Some years ago. | May 11 ... | Do. do. ... | May 12 | Cured | May 23 | May 26 |
| Several yrs. ago. | July 26 ... | Do. do. ... | July 27 | Cured | Begin'g of Oct. | Sept. 5 |
| Two years ago. | July 15 ... | Do. do. ... | July 21 | Cured | Aug. 8 | Aug. 13 |

1, 1875—(64 SALICYLE AND 6 CARBOLIC CASES).

ND COMPOUND FRACTURES.

| | | | | | | |
|---|---|---|---|---|---|---|
| Dec. 6... | Dec. 6 ... | Amput. of thigh—dry salicyle dressing. | Jan. 7 | Cured | Jan. 25 | — |
| Oct. 9... | Oct. 9 ... | Amput. of left leg—same dressing. | Oct. 9 | Cured | Dec. 1 | Feb. 4, 1875. |
| Eight days ago. | Dec. 5, 1874 | Amput. of both legs—same dressing. | Jan. 2 | Cured | Feb. 1 | Feb. 15 |

No. 89. Patient very stout. Tumour in right breast as large as a chestnut, deep-seated, hard. Glands in axilla indistinct. Extirpation of breast and of some suspicious axillary glands. Sutures, drainage, Lister's gauze dressing. Healing process disturbed by constant nervous excitement, slight erysipelas and œdema of left arm, which continued for a considerable time after the wound was healed.

No. 90. Carcinoma of axillary glands. *Vid.* No. 78.

Extirpation, sutures, drainage, Lister's gauze dressing. Wound closed on fifth day, with exception of point of drainage. From sixth to eleventh day, slight erysipelas. Cured on eighteenth day.

No. 91. From fall of a heavy piece of iron, stripping off of the skin, fascia and periosteum of the shin-bone down to the ankle with much laceration of the sheaths of the tendons. For first three days, dry salicyle dressing with bad result; fever, advancing suppuration, commencing lymphangitis, then open treatment with salicyle irrigation. The fever continued, the exposed tendons sloughed, there was suppuration in the ankle-joint and in the muscles of the calf of the leg. Suspicion of osteo-myelitis of the tibia, evening temperatures of 104°. Amputation of femur on thirty-first day. Veins and medulla of removed leg normal. Amputation-wound healed under third dry salicyle dressing on eighteenth day, although the arteries of the thigh were atheromatous and almost chalky.

No. 92. Railway injury. The amputation-wound in the upper fourth of the leg had to be made in soft parts much contused, which retarded the healing process.

No. 93. The skin on both sides for a hand's-breadth above the ankle-joints bluish-black and dead. Gangrene fever. When the gangrenous skin began to get moist, both legs were put up in dry salicyle wadding. The fever ceased in ten days. After the frequently reinforced dressing had been on for eight days, offensive gangrenous smell. Both dressings were then covered with a paste of animal charcoal and starch kept in its place by a gauze bandage. The paste stiffened into a dry crust which confined the ill-smelling products of decomposition. Patient continued in this state in good general health and perfectly free from pain and was placed in a bath on January 3, in which the dressing

| Number. | | Ward. | | Name of Patient. | Age. | Occupation. | Disease or Injury. |
|---|---|---|---|---|---|---|---|
| Current Number. | General Register. | No. | Surgeon. | | | | |
| 94 | 1833 | 3 | K. | N. Carl ... | 21 | — | Compound fracture of foot... ... |
| 95 | 1886 | 5 | B. | M. August ... | 65 | — | Compound fracture of tarsus ... |
| 96 | 1805 | 4 | C. | B. Wilhelm ... | 39 | Labourer ... | Compound fracture of left leg ... |
| 97 | 229 | 4 | C. | Z. Eduard ... | 19 | Labourer ... | Gangrene of both feet from cold ... |
| 98 | 241 | 5 | B. | R. Theodor ... | 28 | Bricklayer | Gangrene of both feet from cold ... |
| 99 | 1774 | 3 | K. | St. Wilhelm ... | 37 | Fireman ... | Compound fracture of carpus and metacarpus. |
| 100 | 253 | 4 | C. | V. Ludwig ... | 20 | Miller ... | Compound fracture of left humerus —fracture of left leg. |
| 101 | 1680 | 2 | B. | R. Frederike ... | 25 | — | Lacerated wound of head—compound fracture of forearm. |
| 102 | 1286 | 5 | B. | H. Hermann ... | 22 | Tailor ... | Spontaneous luxation of femur and contraction of hip-joint. |

came away in a great stench. Gangrenous line well marked everywhere. Both legs amputated on the same day, dry salicyle dressing left on, since the patient was free from fever and pain, for fourteen days. On removing it, some grammes of thin pus free from smell were found confined in the drainage-canals by coagulated blood. These were emptied, washed out with salicyle water, and the drainage-tubes removed. Healing process completed under second dry dressing, without fever.

No. 94. First dressing left on fourteen days, then drainage-tubes removed and healing process completed under the second dressing.

No. 95. Accidentally shot through the foot. Several bones of foot smashed, advancing, suppurative infiltration of leg, high fever. Amputation in upper third of leg, arteries chalky. Parenchymatous secondary hæmorrhage, sloughing of part of skin of flap, and still higher up on inner surface. Hæmorrhage from art. tib. anter. at end of second week, controlled by bull-dog forceps, which was left in position eight days. No fever. Salicyle dressing with irrigation, substituted for dry dressing from third day. At the end of February, a well-granulating wound without sufficient skin-covering, slight necrosis of sawn edge of tibia. A second operation will be unavoidable, to effect the complete healing of the stump.

No. 96. Healing process retarded by retention of the wound-secretions during the first days.

No. 97. Healing process retarded because the skin did not suffice to cover the stump completely.

No. 98. Operation partly within the field of the inflammatory infiltration and thrombosed veins of the skin. Healing process slow.

No. 99. Injury by machinery. Carpus and metacarpus smashed, soft parts of flexor and extensor surface torn up with the exception of some thin bundles. Patient refused

| Commencement of Disease, or Date of Injury. | Day of Admission. | Operation and Mode of Dressing. | Day of the Operation | Result. | | Date of Discharge |
|---|---|---|---|---|---|---|
| | | | | Kind. | Time. | |
| Dec. 10 ... | Dec. 10 ... | Amput. of leg—same dressing. | Dec. 10 | Cured | Jan. 1 | — |
| Six days ago. | Dec. 23 ... | Amput. of leg—dry salicyle dressing; then salicyle irrigation. | Dec. 24 | Granulating stump | — | Under treatmt. Feb.'75. |
| Dec. 5 ... | Dec. 5 ... | Amput. of left leg—Lister's gauze dressing. | Dec. 5 | Cured | Jan. 21 | Do. |
| Fourteen days ago. | Jan. 9 ... | Exarticulation of toes of right foot, etc.—dry salicyle dressing. | Jan. 14 | Almost cured. | — | Do. |
| — | Jan. 12 ... | Exarticulations, with partial resection of cuneiform bone, etc.—same dressing. | Feb. 5 | Almost cured. | — | Do. |
| Nov. 27 ... | Nov. 27 ... | Amput. of humerus — same dressing. | Dec. 15 | Cured | Feb. 1 | Feb. 10 |
| Jan. 13 ... | Jan. 13 ... | Amput. of humerus — same dressing. | Jan. 13 | Cured | Jan. 27 | Still under treatment Feb. 28. |
| Nov. 7 ... | Nov. 7 ... | Disinfection with salicyle water —dry salicyle dressing— Amput. of left forearm— dry salicyle dressing. | Nov. 7 | Cured | Nov. 18 Dec. 14 | Dec. 19 |
| Nine years ago. | Aug. 18 ... | Resection of hip-joint—same dressing. | Dec. 3 | Died | Dec. 28 | — |

permission to amputate. Open treatment with salicyle irrigation. Favourable course at first as regards fever and exfoliation of dead parts. In the second week, while the arm was being placed in position, a sudden attack of pain. The patient pulled the arm violently to him, struggled in his bed, and came to lie upon the arm. From that time, advancing suppuration with high temperature. Amputation of humerus partly in infiltrated tissues. Slow healing process.

No. 100. Cured in fifteen days with two dressings.

No. 101. Attempt at suicide, railway injury. 1. Large flap-wound on anterior half of hairy scalp, extensive exposure of cranium, much dirt pressed into the wound. Cleansing of wound by dissecting out dirty tissues. Washing with salicyle water. Sutures. Dry salicyle dressing. Union by first intention except at some points where there was loss of substance. 2. Smashing of forearm, amputation. Slow healing process without fever.

No. 102. Nine or ten years ago, coxitis ending in spontaneous luxation and contraction, so that the limb was rendered useless for locomotion. Fistulæ in the direction of the former joint. Urine highly albuminous. Decreased resonance in region of liver and spleen. The debilitated patient improved in the hospital, the amount of albumen decreased, and thus I decided, at his urgent request, upon an operation the object of which was, to remove sequestra and to improve the position of the limb. No sequestra were found. The upper part of the femur, the head of which was so shrunk as to be scarcely recognisable, was firmly attached to the dorsum ilii by cicatricial cords. Resection of upper part and extension effected with much difficulty. Dry dressing. Favourable course at first. From the second week, increase of albumen, then anasarca and ascites. Death on December 28. No fever at any time. Post-mortem showed great lardaceous degeneration of spleen, liver, and kidneys. Sawn end of femur presented a narrow rim of sequestrum. Medulla healthy.

| Number. | | Ward. | | Name of Patient. | Age. | Occupation. | Disease or Injury. |
|---|---|---|---|---|---|---|---|
| Current Number. | General Register | No. | Surgeon. | | | | |
| 103 | 1790 | 2 | B. | H. Rosine ... | 48 | Labourer's wife. | Obstinate luxation of humerus ... |
| 104 | 327 | 3 | K. | M. Gottlieb ... | 36 | Labourer ... | Pseudarthrosis of right radius after compound fracture. |
| 105 | 1608 | 1 | K. | N. Hedwig ... | 5½ | — | Rachitic curvature of both legs ... |
| 106 | 294 | 1 | K. | B. Amalie ... | 13 | — | Necrosis of femur ... ... ... |
| 107 | 227 | 1 | K. | J. Moritz ... | 13 | Labourer's son. | Necrosis of tibia ... ... ... |
| 108 | 900 | 1 | K. | M. Ferdinand | 9 | Labourer's son. | Necrosis of tibia ... ... ... |
| 109 | 1642 | 3 | K. | H. Carl ... | 23 | Printer ... | Compound fracture of forearm ... |
| 110 | 1836 | 4 | C. | Sch. Friedrich | 48 | Labourer ... | Compound fracture of leg ... ... |
| 111 | 1691 | 2 | B. | H. Auguste ... | 19 | Servant ... | Compound fracture of both legs, pelvis, and vertebral column. |
| 112 | 1605 | 2 | B. | G. Anna ... | 34 | Labourer's wife. | Incarcerated femoral hernia ... |
| 113 | 210 | 5 | B. | M. August ... | 26 | Joiner ... | Large lacerated wound of head—contused wound of forehead with exposure of cranium. |
| 114 | 1830 | 5 | B. | Sch. Johann ... | 18 | Porter ... | Large contused wound of forehead with exposure of cranium. |

No. 103. Complete luxation of humerus forwards and inwards. Head of humerus firmly situated beyond the coracoid process for six weeks. No attempt at reduction had yet been made. On attempting to give a movement outwards to the head of the humerus by rotation of the bone, the latter broke in the middle although very little force was employed. The attempt then made to liberate the exposed head of the bone failed on account of the shortness and strength of the connections, which I was unable to divide without endangering the vessels and nerves in the axilla. Nothing remained but the resection of the head of the bone to improve the usefulness of the limb.

Very slow healing process with febrile and sub-febrile temperatures, probably from extensive injury to the soft parts during the tedious operation. The fracture was already well healed in the third week. On patient's discharge the arm was still œdematous and could not yet be used at all.

No. 111. Patient is small and insignificant-looking, with the appearance of a girl about fourteen years old. Eleven days ago she was delivered at the lying-in hospital of a well developed child in normal labour. Her recovery was retarded by high fever and delirium with accessions of violence. In one of these she jumped from a first-floor window on to the pavement beneath and was brought the next day (Nov. 10) into the surgical wards. Both ankle-joints difficult to examine well on account of great swelling from extravasation, but fracture was recognisable by crepitation. Gibbous fracture of pelvis. The mental disturbance continued in the form of dread of being pursued, and

| Commencement of Disease, or Date of Injury. | Day of Admission. | Operation and Mode of Dressing. | Day of the Operation. | Result. | | Date of Discharge |
|---|---|---|---|---|---|---|
| | | | | Kind. | Time. | |
| Six weeks ago. | Dec. 1 ... | Resection of head of humerus —same dressing. | Dec. 1 | Almost cured. | Feb. 17 | Feb. 17 |
| 6 mo. ago | Jan. 26 ... | Resection of pseudarthrosis—salicyle irrigation 14 days. | Jan. 29 | Cured | Feb. 15 | Still under treatment, Feb. 28. |
| 4 yrs. ago | Oct. 24 ... | Osteot. tib. d. cuneiformis—dry salicyle dressing. Osteot. tib. sim. simpl. | Oct. 28<br>Jan. 18 | Cured<br>Cured | Nov. 12<br>Feb. 18 | As above |
| About a yr. ago. | Jan. 19 ... | Sequestrotomy—same dressing | Jan. 20 | Cured, with exceptn. of a fistula. | Feb. 2 | Made an out-patient. |
| A year ago | Jan. 8 ... | Sequestrotomy—14 days' irrigation, then dry salicyle dressing. | Jan. 22 | Healing well. | — | Still under treatment, Feb. 28. |
| 3 mo. ago | May 27 ... | Sequestrotomy—dry salicyle dressing. | Nov. 24 | Cured | Jan. 1, 1875. | Jan. 27 |
| Nov. 2 ... | Nov. 2 ... | Salicyle irrigation ... ... | — | Cured | Jan. 15 | Feb. 6 |
| Dec. 11 ... | Dec. 11 ... | Open treatment with salicyle irrigation. | — | Wound healed, Dec. 18 | Fractures not yet firm, Feb. 17. | — |
| Nov. 9 ... | Nov. 10 ... | Dry dressing ... ... ... | — | Fract'rs healed | Feb. 1 | Still under treatment, Feb. 28. |
| Oct. 23 ... | Oct. 25 ... | Internal herniotomy—dry salicyle dressing. | Oct. 25 | Cured | Nov. 27 | Dec. 2 |
| Jan. 5 ... | Jan. 5 ... | Sutures—same dressing ... | — | Cured | Feb. 9 | Feb. 18 |
| Dec. 9 ... | Dec. 9 ... | Same dressing ... ... ... | — | Cured | Jan. 9 | Jan. 14 |

showed itself sometimes by restlessness, sometimes by screaming. Tonic spasms of the muscles of the arms and convulsive closure of the eyelids continued until Nov. 21, and afterwards recurred occasionally for several weeks. There was fever from the time of her admission, with evening temperatures up to 104°—105°. On November 15, there was erysipelas on both legs above the point of fracture, without any wound, and also in the face. From the legs the erysipelas spread upwards to the lower part of the trunk, with constant fever and evening temperatures up to 105° until November 23. On morning of November 23, free from fever. The erysipelas ceased to spread and had disappeared on 28th.

The swelling of both ankle-joints had, in the meanwhile, not quite gone off and now returned and was accompanied by fluctuation. The abscesses, which were situated between the skin and fascia and contained a large quantity of pus, were opened on December 8. Fracture of the ankle-bones was now demonstrable on both sides and one of the calcaneus and talus to be suspected from the change in form. A perforated plaster of Paris capsule had already been applied and was now combined with dry salicyle dressing. The abscesses closed in a few weeks, without suppuration of joints or bones.

On January 3, a fluctuating spot which had been observed for some days and occupied the sacrum was opened. A large quantity of pus and shreds of tissue came away, the sacrum and ilium were found partially denuded and a fissure two inches long in the sacrum.

| Number. | | Ward. | | Name of Patient. | Age. | Occupation. | Disease or Injury. |
|---|---|---|---|---|---|---|---|
| Current Number. | General Register | No. | Surgeon. | | | | |
| 115 | 1841 | 5 | B. | U. Ernst ... | — | Porter ... | Large contused and lacerated wound of head with exposure of cranium. |
| 116 | 317 | 4 | Cr. | H. Traugott ... | 29 | Porter ... | Large lacerated wound of head with exposure of cranium. |
| 117 | 1672 | 3 | K. | K. Carl ... | 44 | Miller ... | Incised wound of head ... ... |
| 118 | 1675 | 5 | B. | R. Friedrich ... | 31 | Labourer ... | Large contused wound of forehead... |
| 119 | 195 | 4 | C. | S. Ludwig ... | 38 | Tailor ... | Incised wound of head ... ... |
| 120 | 1548 | 3 | K. | T. Carl ... | 22 | Labourer ... | Lacerated wound of head ... ... |
| 121 | 1583 | 5 | B. | R. Wilhelm ... | 30 | Locksmith | Contused wound of head ... ... |
| 122 | 1602 | 1 | K. | A. Adolph ... | 13 | — | Large contused wound of head ... |
| 123 | 193 | 3 | K. | M. Carl ... | 18 | — | Incised wound of humerus and scapular region. |
| 124 | 1571 | 2 | B. | B. Johanne ... | 25 | — | Large contused wound of elbow ... |
| 125 | 1894 | 1 | K. | S. Otto ... | 9 | — | Deep wound of forearm ... ... |
| 126 | 1888 | 4 | C. | W. Moritz ... | 16 | Tinman ... | Deep contused wound of forearm ... |
| 127 | 1625 | 2 | B. | K. Sophie ... | 21 | — | Contused wound of hand ... ... |
| 128 | 347 | 5 | B. | P. Traugott ... | 50 | Smith ... | Neglected wound of index finger—phlegmon of hand. |
| 129 | 1863 | 3 | K. | H. Wilhelm ... | 25 | Porter ... | Lacerated wound of back of hand, etc. |
| 130 | 238 | 5 | B. | St. Carl ... | 15 | Messenger | Avulsion of iv. finger—contused wound of palm, etc. |
| 131 | 1812 | 4 | Cr. | K. Ernst ... | 21 | Labourer ... | Wound of index finger ... ... |
| 132 | 300 | 4 | Cr. | Sch. Oscar ... | 21 | Baker ... | Compound comminuted fracture of iii. finger. |
| 133 | 1516 | 5 | B. | K. Johann ... | 55 | Landed proprietor. | Compound fracture of iii. finger, with contused wound of hand. |

Dry salicyle dressing, with which the abscess healed in the course of six weeks, without exfoliation of bone. From January 5—11, a second but slight attack of erysipelas. On January 12, a large abscess situated under the right labium was opened and healed, without antiseptic treatment, in a few weeks. It appears worthy of note that at the times of highest fever, with a temperature of 105° and a pulse from 140—160, the patient could take ample nourishment. From the middle of January, she remained mostly free from fever, and the previous condition of mental derangement changed into a kind of fatuity. Everything is now healed with the exception of a superficial bedsore at the gibbous angle of the pelvic fracture. Both ankle-joints are anchylosed.

| mence-ent of ease, or ate of jury. | Day of Admission. | Operation and Mode of Dressing. | Day of the Operation. | Result. | | Date of Discharge |
|---|---|---|---|---|---|---|
| | | | | Kind. | Time. | |
| 12 ... | Dec. 12 ... | Dry salicyle dressing ... ... | — | Cured | Feb. 4 | Feb. 9 |
| 23 ... | Jan. 23 ... | Sutures, drainage—same dressing. | — | Almost cured. | — | Still under treatment, Feb. 28. |
| 5 ... | Nov. 5 ... | Sutures—*Lister's* gauze dressing. | — | Cured | Nov. 11 | Nov. 11 |
| 6 ... | Nov. 6 ... | Sutures—dry salicyle dressing | — | Cured | Nov. 22 | Nov. 25 |
| 1 ... | Jan. 1 ... | Same dressing... ... ... | Jan. 1 | Cured | Jan. 6 | Jan. 11 |
| 13 ... | Oct. 13 ... | *Lister's* gauze dressing ... | — | Cured | Oct. 23 | Oct. 23 |
| 22 ... | Oct. 22 ... | Dry salicyle dressing ... ... | — | Cured | Oct. 30 | Oct. 30 |
| 24 ... | Oct. 24 ... | *Lister's* gauze dressing ... | Oct. 24 | Cured | Nov. 10 | Nov. 16 |
| 1 ... | Jan. 1 ... | Sutures—dry salicyle dressing | Jan. 1 | Cured | Jan. 10 | Feb. 10 |
| 20 ... | Oct. 20 ... | Moist salicyle dressing ... | — | Almost cured. | Dec. 1 | Dec. 1 |
| 24 ... | Dec. 24 ... | Sutures—dry salicyle dressing | Dec. 24 | Cured | Feb. | Under treatment. |
| 23 ... | Dec. 23 ... | Ligature of radial artery—salicyle irrigation. | Dec. 23 | Cured | Jan. 26 | Feb. 12 |
| 29 ... | Oct. 29 ... | Dry salicyle dressing ... ... | — | Cured | Dec. 19 | Dec. 28 |
| k ago | Jan. 29 ... | Incisions, drainage—sal. irrigation, later on dry dressing | Jan. 29 | Healing well. | — | Still under treatment, Feb. 28 |
| 17 ... | Dec. 17 ... | Open treatment with salicyle irrigation—removal of sequestra. | Feb. 15 | Almost cured. | — | Ditto Feb. 28 |
| 11 ... | Jan. 11 ... | Salicyle irrigation ... ... | — | Cured | Feb. 15 | Ditto Feb. 17 |
| 7 ... | Dec. 7 ... | Exarticulation of ii. phalanx—dry dressing. | Dec. 8 | Cured | Dec. 15 | Dec. 17 |
| 20 ... | Jan. 20 ... | Exarticulation of ii. phalanx of iii. finger—dry dressing. | Jan. 20 | Cured | Jan. 27 | Feb. 6 |
| ... | Oct. 6 ... | Enucleation of iii. finger—dry salicyle dressing. | Oct. 6 | Almost cured. | Oct. 26 | Oct. 26 |

The nursing and treatment of the patient were rendered extremely difficult by her excessively dirty habits, since she passed everything under her, not only when the fever was highest and the mental disturbance greatest, but also later on when free from fever and appearing not altogether inaccessible to remonstrances. The late formation of recesses at four points of fracture was perhaps connected with the antecedent erysipelas, which, as already remarked, did not itself start from a wound.

No. 123. Wound with a knife on the posterior surface of the shoulder-joint, eight inches long and extending into the fossa supraspinata and to the scapula, and laying open, perhaps, the capsule of the joint.

| Number. | | Ward. | | Name of Patient. | Age. | Occupation. | Disease or Injury. |
|---|---|---|---|---|---|---|---|
| Current Number. | General Register | No. | Surgeon. | | | | |
| 134 | 198 | 4 | Cr. | Th. Richard ... | 16 | House porter. | Compound fracture of ii. and ii gers. |
| 135 | 1881 | 2 | B. | B. Ernestine ... | 20 | — | Compound fracture of fingers iv. v. |
| 136 | 1774 | 3 | K. | H. Carl ... | 15 | Grinder ... | Compound fracture of finger a metacarpal bone. |
| 137 | 1751 | 4 | Cr. | H. August. ... | 20 | Baker ... | Compound fracture of right t |
| 138 | 1721 | 2 | B. | L. Minna ... | 21 | — | Compound fracture of some f of both hands. |
| 139 | 1681 | 2 | B. | St. Henriette... | 35 | Labourer's wife. | Compound fracture of phalan of fingers iii. and iv. |
| 140 | 1644 | 4 | Cr. | M. Friedrich ... | 21 | Bricklayer | Compound comminuted fractu thumb. |
| 141 | 1612 | 2 | B. | E. Marie ... | 17 | Servant ... | Avulsion of phalanges ii. and right index finger. |
| 142 | 332 | 5 | B. | F. Theodor ... | 27 | Labourer ... | Compound fracture of toes and tused wound of foot. |
| 143 | 1558 | 3 | K. | G. Gustav ... | 15 | Apprentice | Compound fracture of index and ii. metacarpal bone. |
| 144 | 1547 | 2 | B. | S. Pauline ... | 37 | Shoemaker's wife. | Neuralgia of right index finger |
| 145 | 1576 | 2 | B. | R. Marie ... | 27 | Cook ... | Abscess of face after periosti upper jaw. |
| 146 | 1757 | 90 | B. | B. Carl ... | 22 | Student ... | Abscess of neck after periosti mandibulum. |
| 147 | — | 5 | B. | F. August ... | 23 | Bricklayer | Large, deep abscess of neck |
| 148 | 1641 | 5 | B. | H. Franz ... | 46 | Baker ... | Large abscess beneath musc thorax. |

No. 144. Although on the first occasion (*vide* No. 57) the nerves of the finger we cut away to the extent of 0.2 inches, the evil recurred in the stump. The second exartic lation with resection of nerves has, up to the present time (end of February), be successful.

No. 148. Large fluctuating swelling of right half of thorax, covered by the pectora

| mence-nt of se, or te of jury. | Day of Admission. | Operation and Mode of Dressing. | Day of the Operation. | Result. | | Date of Discharge |
|---|---|---|---|---|---|---|
| | | | | Kind. | Time. | |
| 2 ... | Jan. 2 ... | Amput. of ii. phalanges—dry salicyle dressing. | Jan. 2 | Cured | Jan. 13 | Jan. 22 |
| 22 ... | Dec. 22 ... | Enucleations of fingers—dry salicyle dressing. | Dec. 22 | Cured | Jan. 1 | Jan. 19 |
| 21 ... | Nov. 21 ... | Enucleation and resection—same dressing. | Nov. 21 | Cured | Dec. 24 | Jan. 9 |
| 22 ... | Nov. 22 ... | Enucleation of thumb—same dressing. | Nov. 22 | Cured | Dec. 2 | Dec. 7 |
| 17 ... | Nov. 17 ... | Enucleation of several fingers—same dressing. | Nov. 17 | Cured | Dec. 17 | Jan. 19 |
| 7 ... | Nov. 7 ... | No operation allowed—dry, then moist salicyle dressing. | — | Almost cured. | Dec. 12 | Dec. 12 |
| 2 ... | Nov. 2 ... | Exarticulation of thumb—same dressing. | Nov. 2 | Cured | Nov. 10 | Nov. 18 |
| 24 ... | Oct. 26 ... | Exarticulation of right index finger—same dressing. | Oct. 27 | Cured | Nov. 10 | Nov. 16 |
| 26 ... | Jan. 26 ... | Exarticulation of great toe—sal. irrigation, then dry dressing. | Jan. 26 | Almost cured. | — | Still under treatment, Feb. 28. |
| 17 ... | Oct. 17 ... | Enucleation of index finger, with partial resection of ii. metacarpal bone—moist salicyle dressing. | Oct. 17 | Cured | Nov. 15 | Nov. 27 |
| — | Oct. 13 ... | Enucleation of finger—same dressing. | Oct. 15 | Cured | Oct. 25 | Nov. 9 |
| ys ago | Oct. 21 ... | Incision, drainage—dry salicyle dressing. | Oct. 21 | Cured | Oct. 30 | Nov. 2 |
| do. | Nov. 23 ... | Incision, drainage—Lister's gauze dressing. | Nov. 24 | Cured | Dec. 2 | Dec. 5 |
| do. | Oct. 7 ... | Incision, drainage—dry salicyle dressing. | Oct. 26 | Cured | Nov. 20 | Dec. 17 |
| s ago | Nov. 2 ... | Do. do. | Nov. 3 | Cured | Dec. 5 | Dec. 5 |

major and latissimus dorsi muscles, commencing eight days ago with fever. Temperature on admission, 102.5°. The covering and adjacent soft parts showed great inflammatory infiltration. On laying open the abscess, 300 cubic centimetres of pus and some shreds of tissue were discharged. Drainage by means of two openings opposite to each other. No fever. Cavity of abscess healed on fifteenth, points of drainage on thirty-second day.

| Number. | | Ward. | | Name of Patient. | Age. | Occupation. | Disease or Injury. |
|---|---|---|---|---|---|---|---|
| Current Number. | General Register | No. | Surgeon. | | | | |
| 149 | 1853 | 4 | Cr. | P. Wilhelm ... | 28 | Labourer ... | Abscess of neck ... ... |
| 150 | 1706 | 2 | B. | F. Auguste ... | 26 | Servant ... | Abscess of mamma (bronchitis) |
| 151 | 334 | 4 | Cr. | G. Michel ... | 40 | Porter ... | Subcutaneous abscess of thigh |
| 152 | 1911 | 107 | Cr. | M. Emilie ... | 21 | Servant ... | Patellary bursitis (ulcers of leg) |
| 153 | 1888 | 3 | K. | H. Carl ... | 55 | Wheelw'ght | Phlegmon of forearm from negl wound of elbow. |
| 154 | 230 | 2 | B. | F. Agnes ... | 58 | — | Neglected panaritium, advs suppuration. |
| 155 | 1902 | 5 | B. | M. Bernhard ... | 25 | Labourer ... | Do. do. |
| 156 | 1739 | 2 | B. | B. Emma ... | 19 | Servant ... | Do. do. |
| 157 | 1348 | 1 | K. | Z. Louise ... | 10 | — | Coxitis and congestive abscess |
| 158 | 1732 | 2 | B. | K. Caroline ... | 28 | Weaver's wife. | Adenoma of right mamma ... |
| 159 | 1726 | 3 | K. | H. Franz ... | 47 | Painter ... | Carcinoma of penis and left ing glands. |
| 160 | 301 | 2 | B. | B. Amalie ... | 50 | Weaver's wife. | Corpus alienum ... ... |

No. 156. Severe erysipelas.

No. 159. Congenital phymosis, epithelial cancer of papillary form, extending wit fissure into the corpora cavernosa. Numerous swelled glands in both groins. Amputa tion of the penis on a level with the pubis. Since the opening of the urethra at thi point caused so much inconvenience, the scrotum was divided in the raphe as far as th urethra, the urethra dissected away from the symphysis pubis, a long incision mad through the skin of the perineum behind the posterior angle of the wound to within 1/ inches of the anus, the separated urethra passed through this opening and firmly attache to the edges of the opening by suture. The wound of the penis and scrotum was close by sutures, that in the scrotum drained at its deepest point, and an india-rubber tub introduced into the bladder through the transposed urethra to defend the dry salicyl dressing from being made dirty. A rigor with temperature of 104° in the first night wa probably attributable to the urethral irritation, since the healing of the wound went o undisturbed. The patient is well satisfied with this modification, which may also b adopted satisfactorily in cases of amputation of the penis in its free portion. In such

In the foregoing List, Nos. 1—30, and Nos. 91—111, 51 cases in recorded, of which seven proved

| nce- of s, or of y. | Day of Admission. | Operation and Mode of Dressing. | Day of the Operation. | Result. | | Date of Discharge |
|---|---|---|---|---|---|---|
| | | | | Kind. | Time. | |
| ago | Dec. 15 ... | Incision, drainage—dry salicyle dressing. | Dec. 16 | Cured | Dec. 20 | Dec. 29 |
| 3 ... | Nov. 15 ... | Incision—dry salicyle dressing | Nov. 19 | Cured | Nov. 27 | Dec. 10 |
| ago | Jan. 27 ... | Incision, drainage—same dressing. | Jan. 27 | Cured | Feb. 10 | Feb. 20 |
| 14 igo | Dec. 29 ... | Incision, drainage—Lister's gauze dressing. | Dec. 30 | Cured | Jan. 14 | Feb. 6 |
| iago | Dec. 23 ... | Incision, drainage—salicyle irrigation. | Dec. 25 | Cured | Feb. 14 | Feb. 27 |
| 10 igo. | Jan. 9 ... | Do. do. | Jan. 9 | Cured | Jan. 30 | Feb. 4 |
| ) ... | Dec. 28 ... | Incisions—moist salicyle dressing. | — | Cured | Jan. 17 | Jan. 23 |
| 14 igo. | Nov. 20 ... | Incision—salicyle irrigation ... | — | Cured | Dec. 19 | Jan. 7 |
| ago | Aug. 1 ... | Incision, drainage—dry salicyle dressing. | Nov. 5 | A fistula remains | Dec. 5 | Still under treatment. |
| lo. | Nov. 18 ... | Extirpation of mamma, suture, drainage, dry salicyle dressing. | Nov. 21 | Cured | Dec. 10 | Dec. 10 |
| lo. | Nov. 18 ... | Amputation of penis ... ... | Nov. 19 | Cured | Dec. 14 | Feb. 9 |
| | Jan. 12 ... | Extirpation of inguinal glands —dry salicyle dressing. | Jan. 12 | Cured | Jan. 22 | |
| May Nos. 1 90 | Jan. 20 ... | Extirpation—dry salicyle dressing. | Jan. 23 | Cured | Jan. 30 | Feb. 3 |

ie, the opening in the skin through which the urethra is intended to be led must be med on the under surface of the penis itself.

On account of the very debilitated condition of the patient, the extirpation of the ands in the groin had to be deferred. When this was undertaken, the swellings on the t side were found to have diminished greatly, but unfortunately what remained s found to be infiltrated with cancerous matter. The wounds healed by the first ention.

No. 160. The patient presented herself for the third time, apparently with a return the disease in the cicatrix of the first operation, *vid. ante*, Nos. 78, 90. A painless luration of a peculiar character, 1.2 inches long, in the posterior end of the cicatrix, was reason for operating. The supposed return of the disease proved to be a piece of lia-rubber drainage-tube over which the wound had healed. A capsule of connective sue adhered closely to the tube, the interior of which was filled with soft gelatinous igulated fibrin, which contained numerous colourless blood-corpuscles. There was no ce of any reactive inflammation in the vicinity.

ll of greater amputations, resections, and compound fractures are atal, and are collated in Table II.

TABLE II.—GREATER AMPUTATIONS A

| Current Number. | No. in Table I. | Disease or Injury. | Operation. |
|---|---|---|---|
| 1 | No. 8 ... | Compound fracture of both legs ... | Primary amput. of both legs—secon do. of left thigh. |
| 2 | No. 7 ... | Compound fracture of right leg ... | Primary amput. of right leg—secon do. of right thigh. |
| 3 | No. 2 ... | Pseudarthrosis of left thigh ... | Amput. of left thigh ... ... |
| 4 | No. 3 ... | Gangrenous ulcer of left leg ... | Do. do. ... ... |
| 5 | No. 102 | Inveterate spontaneous luxation of thigh. | Resection of head of femur ... |
| 6 | No. 18 ... | Tubercular caries of both wrists ... | Partial resection of both wrist-joints. 31, 1874.—Amputation of right for Feb. 15, 1875. |
| 7 | No. 22 ... | Compound fracture of left humerus | Primary amputation of humerus ... |

The four first of these seven fatal cases have already been spoken of in Part II.

The details of the fifth case, with fatal termination from uræmia after resection of the head of the femur, are given in Table I. No. 102, and in Table I. No. 18, those of the sixth case, in which death occurred from exhaustion after resection of both carious wrists and subsequent amputation of the forearm. In the fifth case, it was my intention to extract sequestra. This diagnosis proving incorrect, I undertook resection to give the limb a useful position. This serious operative intervention had better have been omitted, for it was followed by increased lardaceous degeneration of the kidneys and thus proved fatal. In the sixth case, the fatal collapse occurred in the night after the operation, in so far unexpectedly as the patient had felt well in the first few hours after the operation, which had cost him scarcely 30 grammes of blood. The post-mortem showed, however, such an extensive amount of tubercular disease in the lungs as I had not diagnosed during life.

I will not assert that in these six cases the fatal results might not have been obviated by a different mode of treatment of the operation-wound, but, without wishing to bias the reader, I do not think it probable.

CTIONS TERMINATING FATALLY.

| Treatment. | Duration of treatment until Death. | Cause of Death. | Remarks. |
|---|---|---|---|
| alicyle dressing | Death on 100th day after the operation. | Hæmorrhage from the bowels. | *Vid.* No. 12 in Part II. |
| o. do. | Death on 123rd day after the operation. | Hydrothorax ... | *Vid.* No. 11 in Part II. |
| o. do. | Death on 23rd day after the operation. | Suppuration of right shoulder-joint. | *Vid.* No. 7 in Part II. |
| o. do. | Death on 28th day after the operation. | Exhaustion ... ... | *Vid.* No. 8 in Part II. |
| licyle dressing | Death on 25th day after the operation. | Uræmia ... ... | — |
| then dry sali-dressing. | Death on 201st day after first operation—on day after second. | Exhaustion ... ... | — |
| s gauze dressing | Death on 30th day after the operation. | Embolic pyæmia ... | — |

So much the more important for the purpose of this treatise is he seventh case, Table I. No. 22, in which death, after amputation of the upper arm on account of a compound fracture, ensued rom pyæmia. It is our only case of pyæmia in the last twelve nonths (March 15, 1874, to March 15, 1875), for even the cases ıot treated antiseptically in that period did not furnish one of )yæmia. So much the more important would it be to know the ircumstances which in this one case were the cause of the pyæmia.

Three injuries existed; that to the right hand does not come nto consideration, since it healed in due time. The second was ı deep, lacerated and contused wound of the left thigh, not impli-ating the bone, was treated by the open method, had free escape nd was in good condition when the pyæmic fever had reached its ıeight; the post-mortem showed no ground for attributing the )yæmic infection to it. Nothing remains, therefore, but the ampu-ation-wound of the upper arm, and that the source of the pyæmia ay there was shown by the suppurative inflammation of the spongy issue of the bony stump under the skin-wound, which was in great ıart healed. If no pyæmic thrombi were found in the larger veins f the stump of the arm, it may well be the case that a longer search vould have discovered some in the small veins; but even in the

opposite case, I regard the osteo-myelitis as sufficient reason for referring the origin of the pyæmia to the stump. The wound had been treated exactly as Lister directs, the surgeon in attendance, Dr. Credé, had, by a visit of several weeks to Lister in Edinburgh, made himself familiar with the latest mode of procedure, and had taken great pains to bring the method into favour. There was certainly no want of care or dexterity, and this must be especially borne in mind, for, however much the idea may be ridiculed, I entertain no doubt that failure often results from under-estimation of the technical instructions; a change of dressing without the protective spray, loosening and displacement of the dressing, leaving the dressing on too long after the secretions have reached the surface, etc. etc., suffice to render a good result doubtful, and those surgeons who regard the whole thing *a priori* as a kind of fashion, or even delusion, run a risk on that very account of attaining bad results. No blame is to be attached to our materials for the Lister dressing, for, although not procured from Edinburgh, they have done good service. And yet the cause lay perhaps in some modification of the manner of applying the dressing. By Lister's own recommendation we had inserted in the dressing a thick layer of dressing-wadding, containing boracic acid, intended to receive and disinfect the fluids from the wound and thereby enable us to leave on the dressing for a longer time. But boracic acid stands, in reality, far below carbolic acid as an antiseptic. If this surmise is not accepted, we might recur to the circumstance that the operation had to be performed within the field of the traumatic extravasation and, therefore, the possibility remains that the disinfection of the wound infected from the beginning by access of the air was insufficient.

I am inclined to suspect that the origin of the pyæmia lay in the insufficient protective power of the boracic acid, and have, therefore, given up its further use for antiseptic dressings.

Of the 44 remaining greater amputations, etc., six cases are first to be deducted which were still under treatment on February 28, 1875, and are given below in Table III.

TABLE III.—CASES OF GREATER AMPUTATIONS, ETC., STILL UNCURED ON FEBRUARY 28, 1875.

| Current Number. | Number in Table I. | Disease or Injury. | Operation. | Mode of Dressing. | Present result of the Treatment. | Duration of Treatment up to Feb. 28, 1875. | Remarks. |
|---|---|---|---|---|---|---|---|
| 1 | No. 95 | Compound fract. of tarsus. | Secondary amput. of leg. | Dry, then moist salicyle dressing. | Stump granulating well, but insufficiently covered. | 67 days | Chalky arteries. Gangrene of upper flap. Secondary arterial hæmorrhage. |
| 2 | No. 13 | Suppuration of shoulder-joint. | Resection of head of humerus. | Moist salicyle dressing. | Cured in 40 days, with good double movement — fistula remains. | 252 days | Slight erysipelas. |
| 3 | No. 107 | Necrosis of tibia. | Sequestrotomy. | 14 days moist then dry salicyle dressing. | Healing well ... | 37 days | — |
| 4 | No. 111 | Fract. of both legs, pelvis, & vertebral column. | — | Dry salicyle dressing. | Everything healed but the fracture of vertebral column not yet firm. | 110 days | Severe erysipelas. |
| 5 | No. 104 | Pseudarthrosis of radius. | Resection of radius. | Moist salicyle dressing. | Operation-wound healed on 17th day. Arm in plaster of Paris. | 21 days | — |
| 6 | No. 106 | Necrosis of femur. | Sequestrotomy. | Dry salicyle dressing. | Cured on 28th day, with the exception of a fistula discharging slightly. Treated as an out-patient | 38 days | — |

After deducting the 7 fatal cases, and the 6 still under treatment on February 28, 1875, there remain 38 cured or almost cured, greater amputations, resections and compound fractures, which are given below in Table IV., with a description of the treatment (whether moist or dry salicyle dressing, or Lister's gauze dressing) and its duration.

TABLE IV.—THIRTY-EIGHT CASES OF GREATER AMPUTATIONS, ETC., CURED OR ALMOST CURED, WITH AN ACCOUNT OF THE TREATMENT AND ITS DURATION.

| No. | Disease or Injury. | Operation. | Current Number in Table I. | | | Duration of Treatment in days until cured. | Duration of Treatment in days until discharged | Duration of Treatment in days until Feb. 28, '75 | Remarks. |
|---|---|---|---|---|---|---|---|---|---|
| | | | Salicyle Dressing. | | Lister's Carbolic Gauze Dressing. | | | | |
| | | | Moist. | Dry. | | | | | |
| 1 | Osteo-sarcoma of leg. | Amput. of thigh. | No. 1 | — | — | 31 | 34 | — | Favourable course. |
| 2 | Do. do. | Do. do. | — | No. 4 | — | 19 | 47 | — | Do. do. |
| 3 | Wound of leg. | Secondary amput. of thigh. | — | No. 91 | — | 19 | — | 53 | Large contused wound, with exposure of tibia —Advancing suppuration under dry salicyle dressing. |
| 4 | Gangrene of feet, from cold | Amput. of both legs | — | No. 93 | — | 30 | 45 | — | Favourable course in spite of retention of pus. |
| 5 | Neuralgia of leg aft. fracture. | Amput. of leg. | No. 5 | — | — | 67 | 122 | — | Union by first intention retarded by continuous twitchings of stump. |
| 6 | Necrosis of tibia. | Do. do. | No. 6 | — | — | 11 | 76 | — | Favourable course. |
| 7 | Compound fract. of leg. | Do. do. | No. 9 | — | — | 62 | — | 127 | Amput. in field of traumatic extravasation, wound, therefore, left half open. |
| 8 | Do. do. | Do. do. | — | No. 92 | — | 53 | 118 | — | Operated on in field of traumatic hæmorrhage. |
| 9 | Do. do. | Do. do. | — | No. 94 | — | 23 | 81 | — | Favourable course. |
| 10 | Do. do. | Do. do. | — | No. 96 | — | 48 | — | 86 | Retention of wound-secretions — slight erysipelas. |
| 11 | Gangrene of foot, fr. cold. | Exarticulation in tarsus. | — | No. 97 | — | 46 | — | 46 | Almost cured. Skin-covering not quite sufficient. |
| 12 | Do. do. | Exarticulation of left metatarsus. | — | No. 98 | — | 23 | — | 23 | Almost cured—operation in field of inflammatory infiltration. |
| 13 | Compound fracture of hand, etc. | Secondary amput. of humerus. | No. 10 | — | — | 39 | 49 | — | Amputated in field of inflammatory œdema. |
| 14 | Do. do. | Do. do. | — | No. 99 | — | 49 | 64 | — | Do. do. |

| No. | Disease or Injury. | Operation. | Current Number in Table I. | | | Duration of Treatment in days until cured. | Duration of Treatment in days until discharged | Duration of Treatment in days until Feb. 28, '75. | Remarks. |
|---|---|---|---|---|---|---|---|---|---|
| | | | Salicyle Dressing. | | Lister's Carbolic Gauze Dressing. | | | | |
| | | | Moist. | Dry. | | | | | |
| 15 | Compound fract. of humerus —fract. of left leg | Amput. of humerus. | — | No.100 | — | 14 | — | 46 | Still under treatment for fracture of leg. |
| 16 | Sarcoma of forearm. | Amput. of forearm. | No. 11 | — | — | 37 | 47 | — | Interrupted cure from restlessness of patient |
| 17 | Compound fract. of hand. | Primary amput. of forearm. | — | No.101 | — | 31 | 36 | — | Favourable course. |
| 18 | Compound fract. of forearm. | Do. do. | — | — | No.23 | 31 | 59 | — | Do. do. |
| 19 | Compound fract. of hand. | Secondary amput. of hand. | No. 12 | — | — | 41 | 41 | — | Amputation in firm infiltrated tissues. |
| 20 | Rachitic curvature of both tibiæ. | Osteotomy of left tibia. | — | No.105 | — | 16 | — | 105 | Still under orthopedic treatment. |
| 21 | Inveterate luxation of shoulder. | Resection of head of humerus | — | No.103 | — | 79 | 79 | — | Almost cured. |
| 22 | Pseudarthrosis of humerus. | Resect. of humerus —suture. | No. 14 | — | — | 125 | 125 | — | Slight erysipelas—late separation of suture and of a sequestrum. |
| 23 | Do. do. | Do. do. | — | — | No.24 | 117 | 129 | — | Long-continued suppuration. |
| 24 | Compound fract. of elbow-joint. | Primary resection, etc. | No. 15 | — | — | 235 | — | 235 | Almost cured — late separation of sequestra. |
| 25 | Do. do. | Secondary resection. | No. 16 | — | — | 93 | 163 | — | Unavailing washing of fresh wound with carbolic acid 1 : 20, and chloride of zinc 1 : 8. |
| 26 | Do. do. | Sequestrotomy of humerus. | — | — | No.25 | 63 | 119 | — | Late separation of sequestrum. |
| 27 | Inveterate luxation of forearm. | Partial resection of elbow. | — | — | No.26 | 35 | 70 | — | Favourable course. |
| 28 | Do. do. | Do. do. | — | — | No.27 | 46 | 61 | — | Do. do. |
| 29 | Synosteosis of wrist-joint. | Resection of radius and ulna. | — | — | No.28 | 24 | 45 | — | Do. do. |

| No. | Disease or Injury. | Operation. | Current Number in Table I. | | | Duration of Treatment in days until cured. | Duration of Treatment in days until discharged. | Duration of Treatment in days until Feb. 22, '75. | Remarks. |
|---|---|---|---|---|---|---|---|---|---|
| | | | Salicyle Dressing. | | Lister's Carbolic Gauze Dressing. | | | | |
| | | | Moist. | Dry. | | | | | |
| 30 | Necrosis of ulna. | Sequestrotomy. | No. 17 | — | — | 57 | 57 | — | Almost cured. |
| 31 | Do. of tibia. | Do. | — | No.108 | — | 39 | 65 | — | Favourable course. |
| 32 | Compound fract. of cranium. | — | — | — | No.30 | 28 | 28 | — | Do. do. |
| 33 | Fract. of femur (knee). | — | — | — | No.29 | 124 | — | 304 | Severe erysipelas. |
| 34 | Compound fract. of leg. | — | No. 21 | — | — | 50 | 112 | — | Favourable course. |
| 35 | Do. do. | — | No. 20 | — | — | 65 | 160 | — | Do. do. |
| 36 | Do. do. | — | No. 19 | — | — | 90 | 104 | — | Advancing suppuration and high fever, in spite of washing out with carbolic acid and chloride of zinc. |
| 37 | Do. do. | — | No.110 | — | — | 70 | — | 79 | Favourable course. |
| 38 | Compound fract. of forearm. | — | No.109 | — | — | 75 | — | 98 | Do. do. |

Amongst the twenty *greater amputation cases* of Tables III. and IV. there is a single one (Table III. No. 1) in which there existed considerable gangrene of the skin, necrosis of the sawn surface of the tibia, and secondary arterial hæmorrhage; the gangrene and hæmorrhage resulting from the atheromatous state of the arteries, the necrosis from exposure of the bone. The gangrene also depended in part upon the filling of the skin-pocket with blood during the first days and the consequent tension of the skin. When so highly atheromatous a state of the arteries exists it appears to me advisable to give up the idea of union of the wound by the first intention, and to adopt the open treatment with salicyle irrigation. For this treatment, the circular incision *à deux tems* would have to be substituted for the flap operation.

In Table IV. No. 3, the femoral artery was rigid but not

chalky, there was no secondary hæmorrhage, and the wound healed in 19 days under dry salicyle dressing.

The Lister ligature with carbolised catgut proved very valuable and did not suppurate out in any case.

The moist salicyle dressing was employed eight times, the dry eleven times, and Lister's gauze dressing once; a transition from the dry to the moist dressing was necessary in one case only, viz. in that mentioned above of gangrene of the flap. The dry dressing recommends itself from being agreeable to the patient where an equally good antiseptic effect is attained. Since it has stood the proof in this respect, it has generally taken the place of the moist dressing with us for amputation-wounds. It also appears to possess advantages over the Lister gauze dressing; it can be closed very completely and applied soft, and requires in ordinary cases to be changed for the first time after about ten days only, when the drainage-tubes are to be removed and healing of the wound occurs under the second or third dressing. If wound-secretions appear on the surface of the dressing, the points are covered with a layer of salicyle wadding. The necessity of changing the dressing frequently in many cases during the first days when Lister's carbolic dressing is used is thus done away with. This favours the healing of the wound.

We may reckon upon this favourable course if the operation is performed in sound parts, the stump has rest and the constitution of the patient is sound, and this holds equally good for the moist salicyle dressing. Such cases are those in Table IV. Nos. 1, 2, 3, 6, 9, 15, 17, further No. 18 with Lister's gauze dressing; but durations of treatment of 11, 14, and 19 days will, however, even with a favourable course, occur exceptionally only, since the healing up of the drainage-canals and the cicatrisation of the points of drainage usually require a period of from 10 to 14 days. Even if the cavity of the wound heal in the first few days and the soft parts remain free from pain and swelling, and there is no fever during the healing process, the complete closure of the wound will not occur before the 30th day, or later.

The healing process becomes irregular in some otherwise favourable cases from retention of the secretions of the wound. The escape through the drainage-tubes is, unfortunately, not so

well secured as in the open treatment of wounds. During the first period, especially, blocking up may occur from coagulation of blood. Tension and fever generally point to this, and the disturbance usually ceases rapidly after the retention has been remedied. That the fever occurs later here I have already pointed out above, but, like pain, it may be wanting altogether. In Table IV. No. 4, double amputation of the legs for frost-bite, the first dry salicyle dressing was left on 14 days because the patient was free from fever and pain. When the dressing was changed, retained pus was found in both amputation-pockets and the drainage-tubes were stopped up. If the pus had not been aseptic, the retention would have made itself known. After the drainage-tubes had been removed and the cavities washed out with salicyle water, the wounds healed under a second and third dressing, but not until the 31st day.

Table IV.:

| | |
|---|---|
| Uninterrupted was the course in . . | 1, 2, 3, 6, 9, 15, 17, 18 |
| Interrupted by retention of secretions . . . . . | 10, 4 |
| „ by restlessness . . . . . | 5, 16 |
| „ from operation being performed in field of injury . | 7, 8 |
| „ from operation being performed in field of inflammatory œdema . . . | 12, 13, 14, 19 |
| „ through want of sufficient skin covering . . | 11 |

The number of the cases is too small for further summaries.

Of the *resections* on account of pseudarthrosis, Nos. 22, 23, synosteosis, No. 29, rachitic curvature, No. 26, compound fractures, Nos. 24, 25, 26, inveterate luxations, Nos. 21, 27, 28, there is nothing particular to remark. It was a success that, with the exception of the secondary resection of the elbow, No. 25, the healing process went on with little or no fever and no advancing suppurations occurred. In no case was the duration of the cure short, in several, Nos. 22, 23, 24, of unsatisfactory length. The case of resection also, Table III. No. 2, which was going on so well in the first weeks, turned out unfavourably on account of the circumstance that a fistula remained.

The *compound fractures* and the injury to the leg, Table IV. No. 2, which were treated conservatively, deserve especial mention.

In the case Table IV. No. 2, the washing out of the wound

with salicyle water and subsequent dry salicyle dressing, in case No. 25 the disinfection with carbolic acid 1 : 20 and subsequent moist salicyle dressing, proved insufficient to effect an aseptic course; in the first case, amputation became necessary, in the second, resection.

The unfavourable course leading to amputation in No. 13 is attributable to a mischance through which the injured extremity was strongly compressed during the suppurating stage.

The compound fracture in the knee-joint, No. 33, ran a very unfavourable course leading to anchylosis, and is not to be regarded as a case treated strictly on antiseptic principles. There existed already on the patient's admission advancing suppuration, and the Lister treatment was begun late, after the air had long had access to the joint.

Unfavourable also was the course in Table IV. No. 36, and Table I. No. 19: in spite of washing out with a solution of carbolic acid 1 : 20 and of chloride of zinc, and subsequent carbolic and then salicyle irrigation, advancing suppuration and high fever occurred.

Favourable, locally as well as generally, was the course in Table IV. Nos. 34, 37, 38, but they were not bad compound fractures (open treatment with salicyle irrigation). The compound fractures of the skull, Table IV. No. 32, and Table I. No. 30, healed very well under Lister's gauze dressing.

The nature of the compound fractures described, and their small number, admit of no certain conclusion concerning the value of the individual modifications of the antiseptic procedure. For the present, and until the results which Volkmann has attained in cases of compound fractures by washing out with strong antiseptic solutions have been made known in detail, I am inclined, in the case of large wounds, apart from any other surgical interference required, to wash out gently and then irrigate with salicyle water, and in the case of small wounds, if no other indications exist, to abstain from all penetration into the cavities for the purpose of disinfection, and to confine myself to salicyle irrigation.

All that can be said of the cases Table III. Nos. 2—6, and Table IV. Nos. 20—38, in favour of the antiseptic treatment is chiefly that in no case did pyæmia occur.

Of 29 *injuries of the hand and foot*, which frequently led to exarticulations of fingers and toes—in all, 31 exarticulations—12 were treated with moist, 11 with dry salicyle dressing, and 6 with Lister's gauze dressing. After full trial of the dry salicyle dressing, I have kept the moist method for those cases in which the laceration of the soft parts is deep and extensive.

The duration of the treatment varies from 5—90 days. Great loss of substance of the skin naturally involves a long duration. The course was in almost all the cases free from fever, and there was neither advancing suppuration nor detachment of cartilages. The 93 days' duration of treatment in case 38, Table I., refers to an old traumatic suppuration of the first metatarsal bone, the exarticulation of which led to tedious exfoliation of the tendons of the peroneus muscle.

Also of the remaining 14 *wounds of the trunk and the extremities* of recent occurrence, their deep and contused character and considerable extent decided me, in four cases, in favour of the moist salicyle dressing (Table I. Nos. 31, 32, 124, 126). Of the remaining 10 cases, 6 were treated with Lister's gauze dressing, 4 with dry salicyle dressing. All the 14 cases ran a favourable course.

The 18 *injuries to the head*, one of which has already been mentioned amongst the compound fractures, went on perfectly well, 7 with dry salicyle dressing, 11 with Lister's gauze dressing. Amongst them there were very large, much contused and dirtied flap-wounds, 10 of them with considerable exposure of bone (3 salicyle, 7 carbolic cases). In none of these 10 cases did exfoliation occur, in none advancing suppuration between the scalp and pericranium. In the cases of Table I. Division II., washing with salicyle water 1:300 was substituted for the solution of carbolic acid 1:20.

In injuries to the head with loss of skin, more frequent opportunity is given than otherwise of observing the healing of wound-surfaces, as it takes place with carbolic or salicyle dressing. If we change the dressing from day to day for this purpose, we are surprised to find that on the fourth or fifth day the wound-surface presents the appearance of a fresh wound-surface, and that there is but little suppuration. This state of things is explained by the fact that beneath the antiseptic dressing the uppermost layer of tissue retains its vitality. In

the other methods of treating wounds the surface dies, the wound assumes a dirty appearance, then gradually becomes cleansed, and if the separation of the dead tissues is completed from the eighth to the tenth day, and the wound cleansed, the granulation taking place under this layer is already effected. What otherwise occurs hidden under this layer is here completed before our eyes, we see the tissues at work as it were. An immediate skinning over of the wound, as is sometimes spoken of, in cases where the skin fails in its entire thickness, I have never seen. The hollow wound gradually fills up through the growth of the granulations, and then closes in the usual way by advancing cicatrisation from the edge. It appears probable to me that the absence of the superficial mortification benefits the exposed bone, and that the formation of a sequestrum of the crust, so frequent under other circumstances, does not take place, because the soft parts contained in the exposed crust escape mortification. We are also reminded of subcutaneous processes of the healing of bone by the state of the sawn surface, of which mention is made in the post-mortem conditions in Table I. The wound had been for three weeks in a state of suppuration. The granulating medulla projected slightly beyond the sawn surface, and was already covered with a hard layer of bone, while in other cases the bony closure of the medullary canal does not occur until after the closure of the wound.

Seven cases were treated with *advancing suppuration* of the hand or foot in consequence of paronychia and slight injuries. In all, washing out, incision, drainage, and antiseptic dressing proved sufficient to check the suppuration and remove the fever. The washing out was effected chiefly with salicyle water; 6 cases were treated with moist salicyle dressing and irrigation, 1 with Lister's gauze dressing. Duration of treatment 22—66 days.

In the 17 *cases of abscess*, the dry salicyle dressing gradually took the place of the carbolic dressing. No. 73, Table I., a serious case of congestive abscess after periostitis of the os pubis had to be discharged *uncured* at his own request. A fistula with moderate discharge of healthy pus still remained in three cases of congestive abscess of the hip, Table I. Nos. 74, 75, and 157, and in the non-inflammatory abscess of unknown origin, Table

I. No. 69 (209 days). Ætiologically worthy of note are Table I. 61, abscess after acute rheumatism, and Table I. 70, deep, acute abscess of the thigh after advancing paronychia of the hand, with a duration of treatment of 18 and 12 days respectively.

The duration of treatment in the cases cured varies from 5—32 days. Single or numerous incisions, washing out with salicyle water, drainage, dry salicyle dressing, proved beneficial in 9 cases. Lister's gauze dressing was used in 8 cases.

In the large, stationary, subcutaneous abscess, Table I. 151, the entire cavity was laid open by means of large incisions, the granulating lining removed as a connected membrane with the handle of the scalpel, and the cavity washed out with salicyle water. The cavity healed by the first intention with the exception of the point of drainage and a small pocket.

Suppurative, præpatellary *bursitis* and *peribursitis* came three times under treatment, Table I. 62, 63, 152. Two of these cases ran a favourable course, duration of treatment 22 and 13 days. In the third, Table I. 63, the cure was retarded by severe erysipelas, which led to repeated giving way of surfaces already healed. Duration of treatment 52 days. The treatment was the same as for abscesses, twice moist salicyle, once carbolic dressing.

The case of *acute suppuration of the knee*, Table I. 64, was opened, washed out with salicyle water, drained and irrigated. The case went on well to the 20th day. Then erysipelas, which caused death in 8 days.

The very severe case of *empyema*, Table I. 65, was completely cured after a slightly secreting fistulous opening, treated with dry salicyle dressing and washed out daily, had continued until the end of February.

Of the 18 *operations for tumour*, an extirpation of the breast for cancer, Table I. 87, proved fatal from an acute suppuration of the knee-joint, commencing in the first day after the operation while the appearance of the wound continued healthy. (Lister's gauze dressing.)

The other 17 cases, of which, it is true, eight, Table I. 1, 79, 80, 81, 82, 90, 158 and 160, were slight, terminated favourably. The healing process was much interrupted in the case of cystic struma, Table I. 76, through complication with erysipelas.

In two cases of *hydrocele*, Table I. 82 and 83, the tunica vaginalis was divided longitudinally, cauterised by painting

it once with carbolic spirit (1 : 2), united by a double suture, deep and superficial, and drained (in 83 on both sides). A similar plan was adopted in a case of *hygroma patellæ*, Table I. 81. In a third case of hydrocele, the greatly thickened tunica vaginalis was extirpated together with the testicle, and the scrotal wound united by double suture and drained. A large *adenoma of the testicle*, Table I. 85, was operated upon in a similar manner. These five cases ran their course free from fever, without any swelling of the scrotum worth mentioning, and with slight suppurative secretion under Lister's gauze dressing. In a sixth case, Table I. 159, of amputation of the penis on a level with the symphysis pubis for carcinoma, with splitting of the scrotum to bring the opening of the urethra into the vicinity of the perineum, the wound in the scrotum healed equally well with dry salicyle dressing. The duration of treatment in these six cases was 25, 24, 29, 20, 16 and 25 days respectively.

Considering the great liability of the cellular tissue of the scrotum to septic infiltration, I regard these six cases as furnishing strong evidence in favour of the antiseptic treatment.

Also Table I. 160, may be quoted as an instance, since an overlooked piece of drainage-tube left in the operation-wound at once became enclosed in a sac.

If we examine once more the 160 cases in reference to *incidental wound-diseases*, we find one case of embolic pyæmia (Table I. 22), and three cases of *acute suppuration of a secondary nature*, but not connected with embolic pyæmia, two of which, Table I. 2 and 87, proved fatal, and the third, Table I. 70, terminated favourably.

Contrasted with this small number we find thirteen cases of *erysipelas*, of which one proved fatal.

These 13 cases may be arranged as follows.

Erysipelas occurred:

With Lister's gauze dressing (out of 51 cases).

| | | | |
|---|---|---|---|
| In Table I.—29. | Compound fracture of thigh | .. | severe. |
| 74. | Abscess of thigh | .. .. | slight. |
| 75. | Do. do. | .. .. | do. |
| 88. | Extirpation of breast | .. .. | do. |
| 96. | Amputation of leg | .. .. | do. |

WITH MOIST SALICYLE DRESSING (out of 60 cases).

| | | |
|---|---|---|
| In Table I.—13. | Resection of humerus .. .. | slight. |
| 63. | Suppurative præpatallery bursitis, | severe. |
| 64. | Suppuration of knee .. .. | fatal. |
| 76. | Opening of strumous cyst .. | severe. |
| 156. | Advancing paronychia .. | do. |

WITH DRY SALICYLE DRESSING (out of 49 cases).

| | | |
|---|---|---|
| In Table I.—159. | (?) Amputation of penis .. | slight. |
| 111. | Fracture of both legs, etc. .. | severe. |
| 65. | Empyema .. .. .. | slight. |

We find ourselves here in a dilemma, to extricate ourselves from which, further investigations are necessary. Either Lister's treatment excludes the atmospheric ferments, in which case the erysipelas cannot depend upon them, or the erysipelas does depend upon them, in which case the Lister treatment does not exclude them. I incline to the first assumption and regard the bacteria met with in erysipelas as accidental concomitants of the erysipelatous process.

I do not know whether bacteria have already been found in cases of erysipelas occurring with antiseptic dressing; in all other cases, the bacteria may have penetrated through the open wound, to multiply in the soil provided for them by the erysipelas, and to disappear again with the cessation of the latter.

This is at present a mere hypothesis which I cannot support by anatomical facts. I have omitted hitherto, in cases where the erysipelas commenced during the use of antiseptic dressing, to examine the tissues and lymph- and blood-vessels in reference to the presence of bacteria in them. The fact that in examining the blood and secretions of the wounds, I have in no case found bacteria in them is, as I must admit, not decisive.

If, in accordance with my surmise, the erysipelas in cases treated antiseptically remains free from bacteria, the assumption would lie very near at hand that an action of the oxygen of the atmosphere upon the secretions of the wound suffices to produce an erysipelas poison partaking of the nature of a ferment, but not organised. The *acute secondary suppurations* mentioned

above, of which three cases occurred, had, perhaps, a similar origin.

It does not come within the object of this treatise to discuss more closely the nature of traumatic erysipelas, but I wish, while reserving details for another occasion, to mention here that my therapeutic attempts to bring erysipelas to a standstill have furnished only doubtful results. I have probably tried everything that has been recommended on competent authority; for the present I confine myself locally to rubbing in oil and applying ice, to which are added, when there is a continuance of high temperatures, cooling baths as in the treatment of typhus. Besides this, severe cases of erysipelas were treated with camphor in Pirogoff's doses, but subcutaneously. Thus in the case, Table I. 11, thirty subcutaneous injections of nearly four grains (0.25 grm.) were made in five days.

On the ground of the experiences given above we may, I think, adopt the view of those who see in the Lister treatment of wounds a pretty certain defence against *pyæmic* infection.* Fresh wounds, especially operation-wounds, heal in a great majority, when no particularly unfavourable conditions exist, without fever. The method favours union by the first intention, and if the wound-secretions are retained, the ill effect is not considerable; advancing suppurations, etc., generally take a good turn in a few days if proper provision is made for drainage; necrosis of sawn surfaces and of exposed portions of the cranium is a rare occurrence; equally rare, thanks to the aseptic course and the good qualities of the Lister ligature, are secondary arterial hæmorrhages.

As regards the question of salicylic acid, a part of the object of this treatise has been to call forth experiments on a larger scale. The antiseptic effect of the salicyle dressings employed by me appears to me quite as reliable as that of the Lister

* In reference to hospital gangrene, the favourable results attained in the Munich Hospital, quite recently made known by Professor von Nussbaum, speak strongly in favour of the Lister carbolic treatment. I do not know whether salicylic acid has already been used anywhere as a prophylactic against hospital gangrene. In the Magdeburg Hospital also, as I have heard while this treatise was in the press, the Lister treatment, introduced two years ago by Dr. Hagedorn has rendered excellent service in unfavourable hygienic circumstances.

carbolic dressings. At the same time, the salicylic acid has two advantages: it is less irritating and it is not volatile. It can also be combined in larger quantity with the dressing, and the latter can be left on longer than the carbolic dressing without endangering the result. A further recommendation with many will be the fact that salicylic acid is free from smell.

For those of my readers who are disposed to make such experiments, but have neither time nor inclination to extract what is technical from the foregoing pages, I venture, in the following, concluding part, to give some short directions for the employment of the salicyle dressing.

## IV.

A few words to begin with concerning the salicyle preparations required for this mode of dressing, of which a good supply must always be in readiness. The *salicyle water*, i.e. a solution of salicylic acid in water in the proportion of 1 : 300, naturally presents no difficulty for its preparation, but it is different with the salicyle-wadding, which is employed in the proportions of 3 per cent. and 10 per cent.

The apothecary of the Jacob's Hospital, Mr. Blaser, of whose willingness and skill in assisting me in my experiments with salicylic acid I cannot speak too highly, has recommended the following as the best mode of preparation:—

(A) 3 PER CENT. SALICYLIC ACID DRESSING-WADDING.

750 grammes of salicylic acid are dissolved in 7,500 grammes of spirit of sp. gr. 0,830, then diluted with 150 litres of water at a temp. of 158°—176° Fahr. and 25 kilogrammes (55 lbs.) of cotton-wool freed from grease soaked in the mixture.

(B) 10 PER CENT. SALICYLIC ACID DRESSING-WADDING.

1 kilogramme of salicylic acid is dissolved in 10,000 grammes of spirit of sp. gr. 0,830, then diluted with 60 litres of water at a temp. of 158°—176° Fahr. and 10 kilogrammes (22 lbs.) of cotton-wool freed from grease soaked in the mixture.

The soaking of the cotton-wool can be done best in a roomy, flat wooden vessel, in which the individual layers of the wool have convenient space, and it is to be recommended that only small quantities (2—3 kilogrammes) should be dealt with at once, since in this manner only is the uniform dis-

tribution of the solution of salicylic acid possible. This process is soon ended and can therefore be repeated frequently in a short time.

The placing of the cotton-wool in the hot solution of salicylic acid is carried out in such a manner that both in the case of the solutions *a* and *b*, an appropriate quantity of cotton-wool is put in singly, layer by layer, with slight pressure, so that each layer shall become fully saturated before another is added. When the appropriate quantity of cotton-wool has been put in and become thoroughly saturated with the solution of salicylic acid, the whole is to be turned in such a manner that the lowest layer shall come to lie at the top and remain so a short time (about ten minutes), so that the solution of salicylic acid may be as uniformly distributed as possible. The cotton-wool must then be taken out singly in layers and thoroughly cooled in quantities not exceeding 3 kilogrammes in one pile, during which time the salicylic acid becomes crystallised. The wadding thus impregnated must be spread out after about 12 hours in a moderately warm place to dry. It should not be hung up for this purpose, since the settling down of the fluid would interfere with the uniform distribution of the salicylic acid.

It is not to be expected that the salicylic acid will crystallise uniformly in each part of the wadding, and also, after the drying of the wadding, the distribution of the acid may be rendered unequal by handling. We shall find, therefore, in some parts of the 3 per cent. wadding more, in others less than 3 per cent., and the same will apply to the 10 per cent. wadding. An excess will not do harm in any case, but the deficiency should not exceed 1 per cent. in the former case, or 3 per cent. in the latter, and the deficiency in some places may be compensated by excess in others.

It is evident that we require some means of ascertaining the amount of free salicylic acid contained in purchased salicyle-wadding.

The process recommended by Prof. Kolbe for determining the volumetric quantitative amount of salicylic acid in dressing material is the following:—

We require for this test an aqueous potash or soda solution of known strength. We may best employ for this purpose a 10 per cent. standard solution of soda, of which 1,000 cubic centimetres neutralise 13.8 grammes of salicylic acid (*i.e.* $\frac{1}{10}$ of its molecular weight).

A weighed quantity of dressing material (3—5 grammes) is placed in a roomy porcelain dish and heated with about 100 times the quantity of water coloured red with good tincture of litmus. To this hot, coloured solution, in which the dressing material still lies, we add from a burette

the above standard solution of soda, with constant stirring, until the fluid has assumed a permanent blue colour.

The following experiments show the degree of accuracy:

### Dressing-wadding I. (3 per cent.)

*Experiment* I.—Five grammes of wadding, treated with water as directed, required 15.1 cubic centimetres of the standard solution of soda. That corresponds to 0.2083 grammes of salicylic acid (=15.1 × 0.0138) or 4.16 per cent.

*Experiment* II.—Five grammes of the same wadding, treated similarly, required for the neutralisation of the salicylic acid contained therein 15.7 cubic centimetres of the standard solution of soda, corresponding to 0.2194 grammes=4.39 per cent. of salicylic acid.

### Dressing-wadding II. (10 per cent.)

*Experiment* I.—Three grammes of wadding required 17.7 cubic centimetres of the standard solution of soda, corresponding to 0.2442 grammes=8.14 per cent. of salicylic acid.

*Experiment* II.—Three grammes of the same wadding required 17.3 cubic centimetres of standard solution of soda, corresponding to 0.2387 grammes=7.96 per cent. of salicylic acid.

It has already been mentioned that the *salicyle wadding* does not allow the wound-secretions to pass off so well as Lister's *carbolic gauze*, and thus it happens that if the dry salicyle wadding-dressing has been left on 8—14 days, retained pus is generally found between the wound and the dressing in varying quantities. On that account I made various attempts to replace the wadding by another more permeable dressing material. Hemp, flax, shavings, etc., did not answer the purpose. While I was occupied with these experiments, Dr. v. Mosengeil paid a visit to the Leipzig Hospital and directed my attention to *jute*. Jute is the inner bark of various species of *corchorus*, especially of *corchorus capsularis*, which is regularly cultivated in Bengal, and has been employed largely for about twenty years in Europe for making coarse and fine mats, covers, etc. Dr. v. Mosengeil was so kind as to provide us with a quantity of jute from the manufactory at Bonn. This jute was not worked up, as is usually the case, with train oil, but with water, and proved to be free from dirt. It was combined with 3 per cent. of salicylic acid and the attempt made at the same time to prevent the salicylic acid from flying off as dust by an addition of 20 per cent of glycerine.*

* An addition of glycerine will probably also prove useful in the preparation of salicyle-wadding.

The preparation was carried out as follows:—2.500 grammes of jute were placed in a solution consisting of

75 grammes of salicylic acid,
500 „ glycerine, 1:23,
4,500 „ water,

and the solution raised to a temperature of 158°—176°.

In this manner we obtained a soft, pliable dressing material resembling flax and giving off but little dust, which took up thick pus entirely when left on for nine days and distributed it uniformly. At the same time the dressing remained free from smell and showed everywhere with chloride of iron the salicylic acid reaction.

On account of this uniform distribution of the secretions of the wound in the jute dressing it is not necessary to form the inner layers of the dressing of 10 per cent. salicylic wadding, and the 4 per cent. jute material suffices for all cases.

I believe that, for all large dressings of wounds suppurating freely, jute will take the place of wadding, although it is not to be compared with the latter for softness and delicacy of fibre.

It is also of practical importance to ascertain the relative cost of these different dressings. I therefore give that, according to present prices, for the dressing after an amputation of the thigh.

| | |
|---|---|
| Lister's carbolic dressing .. .. | 2.35 Marks. |
| Dry salicyle dressing .. .. .. | 1.52 „ |
| Dry salicyle jute dressing .. .. | 0.92 „ |

The greater cost of the Lister dressing is further increased by the more frequent renewal necessary as compared with the wadding or jute dressing. The cheapest, in itself already and still more because, as I believe, it may be left on longest without danger of an obstruction of the wound-secretions, is the *salicyle-jute-dressing*. But the experiences of the jute dressing are not yet sufficiently numerous to enable us to speak positively in its favour.

I regard it as of little importance whether we use *spray* of carbolic water 1:50, or salicyle water 1:300. Many give the preference to carbolic acid because it does not cause coughing and sneezing and because it disappears thoroughly from the

clothes again by evaporation. I prefer the salicyle water because it irritates the wound less.

For the *disinfection of the neighbourhood of the operation* and of the hands used during the operation we may employ equally well salicyle water 1:300, or carbolic water 1:20. On the other hand, salicyle water is not adapted for the disinfection of the instruments, since the acid oxidises the steel. The sponges should be kept until wanted in carbolic water 1:20, and every time after being used, washed out in a large quantity of warm water and put back into the carbolic water. They may also be kept in salicyle water.

*Lister's protective* is not required, since the salicyle dressing irritates less than the carbolic. *Lister's macintosh* also, which he introduces into the outer layer of the gauze dressing, is superfluous.

To facilitate the escape of the secretions into the wadding and to prevent at the same time the adhesion of the dry salicyle dressing, we first place a piece of perforated gutta-percha paper or oiled silk upon the wound, or a piece of Lister's antiseptic carbolised gauze may be used with advantage for this purpose.

To prevent interruptions, it is well to prepare beforehand everything that may be required for the antiseptic procedure.

In addition to narcosis from chloroform and Esmarch's artificial emptying of blood-vessels, there must be prepared for the operation and dressing:

For *general purposes:*

Several quart bottles filled with carbolic water 1:20, 1:50, and salicyle water. Waterproof tissues to defend the patient from wet.

A bucket for the reception of the fluids after use.

For the *preparatory steps:*

Soap, water, spirits of wine, turpentine, according to circumstances, for cleansing the neighbourhood of the operation, razors, and a strong brush (large nail brush).

A vessel with salicyle water, or carbolic water 1:20 for the disinfection of the neighbourhood of the operation and of the hands engaged in the operation.

For the *operation :*

A dish with carbolic water 1 : 20 in which to place the instruments.

A similar dish or one with salicyle water for the sponges

A small plate with carbolic oil (1 : 10) for Lister's catgut.

Lister's spray apparatus, with a reserve one.

For the *dressing :*

A wound-syringe with canulæ of various calibres.

A dish with salicyle water in which to place drainage-tubes, gutta-percha, or oiled silk.

Strong and weak salicyle-wadding cut into convenient strips for the dressing.

Bandages of gauze unprepared, woven elastic bandage, pins of various kinds.

We commence with the administration of chloroform and, when feasible, with Esmarch's bloodless method according to the instructions of the inventor. We next proceed to cleanse the neighbourhood of the operation, *i.e.* we shave, wash away thoroughly dirt, crusts, remains of plaster, etc., with soap, water, spirits of wine, or turpentine. The cleansed neighbourhood of the operation is then to be brushed smartly for some minutes with a nail-brush repeatedly dipt into salicyle water, or carbolic water 1 : 20. After this, when the hands have been disinfected once more and the spray set going, the operation may be commenced.

When one of the greater amputations has been performed, the search for the larger arteries whose position is known presents no difficulty, and the smaller ones are most easily found by observing where the greater muscular septa cross each other. Since the patient is narcotised and the surface of the wound free from blood, this may be carried out without hurry and without violence to the wound. In an amputation of the leg, 12 to 18 arteries may be thus discovered, in one of the thigh, 18 or more. The process is, however, somewhat tedious, and except where it is desirable to lose the smallest possible quantity of blood, the elastic ring may be removed after the tying of the larger branches and the rest secured in the bleeding condition. The two extremities of the catgut ligature, when we are convinced that it is firmly placed, are to be cut off short, and in like manner the portion of artery projecting beyond the

ligature, except a short piece. When the hæmorrhage has ceased, the edges of the wound are fitted closely to each other, and any superfluous portions cut away, so that their union may be effected without tension but also without folds. The wound is then closed exactly with a disinfected silk suture, deep and superficial alternately, with the exception of the two corners most dependent after the stump has been put into position. After this, the wound is to be syringed out from the two corners with salicyle water, a drainage-tube with numerous lateral openings introduced at each corner as far as the bone and held in position by a suture, and the projecting end cut off on a level with the wound. After a further washing of the wound with salicyle water, until the fluid passes off almost colourless through the drainage-tubes, the line of the wound is to be covered with a strip of perforated gutta-percha or carbolic gauze of three fingers' breadth; over this is placed a layer of 10 per cent. salicyle wadding as thick as a finger, then one of 3 per cent. wadding two fingers' thick and reaching a hand's-breadth upwards from the stump. These layers of wadding are to be pressed gently on to the stump with a gauze bandage in the usual manner for an amputation dressing.

The spray is stopped as soon as the wound is covered with salicyle wadding. The patient is then placed in bed and watched. If he complains that the dressing is too tight, the gauze bandage must be cut longitudinally and secured loosely by pieces of gauze.

Temperatures of 101° render it desirable to remove the dressing and inspect the wound, especially if the patient complain of pain in the wound. If there is neither pain nor fever, the dressing is left on until it appears necessary to remove the drainage-tubes. Points on the surface of the dressing at which secretions appear are to be covered with salicyle wadding. If all goes on well, the dressing is changed for the first time, in greater amputation-wounds, on the 8th—10th day, the drainage-tubes are removed, and the second dressing is left on until the wound is healed. If there is a moderate amount of retention of wound-fluids, the drainage-tubes are removed and replaced by fresh ones, which serve for washing out the wound with salicyle water, and dry dressing applied again. If the wound

has become filled with coagulated blood, I open the middle portion of the suture, and adopt the moist dressing with irrigation; also when gangrene of the flaps occurs.

If we amputate with highly atheromatous (chalky) arteries, it is better to give up from the first the hope of union by the first intention and to leave the wound open, placing over it a thin layer of weaker, moistened salicyle wadding and irrigating with salicyle water.

In the case of *greater resections and other operations* the procedure is the same *mutatis mutandis*. In all cases of deep wound-cavities it will be well, if the operation-wound does not itself permit of escape to the most dependent point, to make a precautionary opening at that point and to introduce a drainage-tube. It is desirable, for instance, on closing the wound after extirpation of the breast and axillary glands, to make an opening in the posterior wall of the axilla and introduce a drainage-tube, to prevent infiltration beneath the latissimus dorsi.

If suppuration occurs in such deep wound-cavities, we should not withdraw the drainage-tube all at once, but a little more each time the dressing is renewed.

In *amputations of fingers and toes*, we may omit the drainage, and I have sometimes left the arteries untied and applied the dry salicyle dressing before the elastic ring was removed. The result was, however, not always satisfactory, and the plan is not to be recommended.

In *wounds of the head*, much depends upon careful removal of all particles of dirt, and if sand, etc., is pressed into the surface of the wound, it is well to dissect off the layer of tissue, as also to cut away edges much contused. An exactly-closing suture, or where this is not possible on account of loss of skin, sutures, to keep the parts in position, are to be recommended. Larger flaps should be drained at their most dependent points. The fixing of the dry dressing may be effected by a few lightly-drawn turns of an elastic bandage. Such elastic strips are also useful for dressings applied to the trunk.

Greater *compound fractures* are treated on the open plan with *salicyle irrigation*. The rest of the treatment of the fracture differs according to its position, nature, and the stage at which it comes under treatment: *location upon chaff pillows, padded*

*splints*, Heister's *splint*, von Brun's splint apparatus, *extension with adhesive plaster* with Volkmann's cradle, *perforated plaster of Paris bandage*, etc., were employed, and with all of these, including the plaster of Paris bandages, irrigation may be combined.

In *greater open wounds*, after the other indications for extraction of splinters, resection, etc., have been fulfilled, I consider a gentle washing out with salicyle water advisable, and a precautionary drainage may be of use. In slight cases, with but little injury to bone and a small opening, all penetration into the wound should be avoided.

In large wounds it is advisable to spread a thin layer of salicyle wadding or gauze over them so that the irrigation may reach every part of the wound. The irrigation is effected in the usual way with Esmarch's *irrigator*, or if this is not available, the wound must be watered from time to time from some convenient vessel. The unpleasant circumstances associated with irrigation are known. In wounds of the thigh, shoulder, neck and trunk, it will always be difficult to carry off the irrigation-fluid permanently, by means of waterproof cloths, into a vessel placed near the bed without wetting the bed itself. In other injuries of the extremities this is more easily effected.

The skin, as far as it is exposed to irrigation, is defended against maceration by rubbing in palm oil from time to time. If abscesses form with retention-fever, we wait for distinct signs thereof to open them under antiseptic spray and, where necessary, to drain them. When the time for abscesses is past and the cavity of the wound in a good state of granulation, we may proceed to use the dry salicyle dressing. This moment will, in the majority of cases, fall in the fourth or fifth week. The simultaneous employment of ice-bags will be of great use in many cases.

In cases taken in with *advancing suppuration* and insufficient drainage, mostly accompanied by high fever, we begin by washing out the suppurating cavity with salicyle water, then under the defence of salicyle spray the necessary openings are made and provided with drainage-tubes, the part operated on fixed in a convenient and advantageous position, and irrigation

with salicyle water commenced. Here also the dry dressing takes the place later on of the moist.

In *smaller abscesses*, or in *abscesses from congestion*, one opening and one drainage-tube usually suffice, the latter being withdrawn in proportion as we find the cavity of the abscess closed. In *large, deeply-seated abscesses*, especially if mortified shreds of tissue exist, we should make a large incision at the most prominent point, wash out with salicyle water, and then make one or more additional openings according to circumstances, at the most dependent points, to effect, if possible, a complete drainage of the large cavity. I consider it better to provide each opening with a tube extending to the middle of the cavity than to introduce tubes which traverse it.

Washing out with salicyle water is useful so long as the walls of the cavity do not adhere to each other.

In *large superficial abscesses* of long standing and stationary character, I lay the abscess open entirely, and scrape off carefully the existing layer of granulations. The wound is then closed carefully with the exception of a drainage opening, and compressed with salicyle wadding.

For *suppuration of abscesses* the dry dressing will almost always suffice.

In all these smaller operations, the neighbourhood of the operation must be prepared in the same manner as in larger ones, and the antiseptic spray kept up whenever the air has access.

These short remarks may suffice, as nearer details are given in the account of the individual cases recorded in the Tables.

In conclusion, I will allude to one question which has doubtless presented itself to many of my readers, and which leads us back to the commencement of this treatise, the question, namely, whether salicylic acid is destined to play a part in *military surgery*.

So far as concerns the *first* days, or even weeks after a battle, and active treatment on the spot, there is no opportunity for Lister's treatment of wounds, whether carried out with carbolic or salicylic acid. The treatment requires such an amount of labour and watching as is impossible at such a time. It would perhaps be advisable, however, leaving the strict observance of Lister's method out of consideration, to substitute for the lint

which has hitherto been employed in dressings, salicyle wadding-dressing, and to irrigate or moisten otherwise with salicyle water the wounds treated by the open method.

At a later period, however, when the lazarets have been got into full working order, and especially if it prove that salicylic acid, like carbolic acid, is a prophylactic against hospital gangrene, it may serve to facilitate the introduction of Lister's method into military lazarets, and thereby acquire a permanent place in the practice of military surgery.

REMARKS UPON

# EXUDATIONS IN THE NEIGHBOURHOOD OF THE FEMALE GENITAL CANALS.

BY

PROF. OTTO SPIEGELBERG,

OF BRESLAU.

GENTLEMEN,—In the course of the session you have seen, especially amongst the out-patients, such a large number of cases in which the uterus or its immediate neighbourhood presented inflammatory thickenings, nodes (tumours) and contractions, that you must now admit the truth of the old opinion that these affections, independently or as mere complications of other morbid conditions of the genital organs, form the largest contingent in obstetric practice. The knowledge of the pathological importance of these exudations, however, is of more recent date. Whilst up to the time of Lisfranc the inflammatory state of things in the genital tract and its surroundings was comprehended under the word "Engorgement," and, later, Metritis and Oophoritis were the diagnoses, it is to French authors, especially Marchal de Calvi, Monat, and Bernutz and Goupil, to whom we are indebted for a clear idea as to the state in question. And at the same time two opposite opinions arose, one of which (Bernutz and Goupil's especially) mainly transferred the inflammation to the serous membrane enveloping the genital apparatus and constituting the recesses and folds, and said that pelvic peritonitis was the predominating affection; whilst the other shifted those processes into the connective tissue in which the genital canal lies, and which forms the parenchyma of the broad ligaments, and held that a

phlegmon of the pelvis was the commonest cause of the exudative swellings and nodes. Whether perimetritis or parametritis is the more frequent affection we are not all agreed at the present time, and it may be difficult to arrive at a decision. These peri- and para-uterine affections will for a long time to come offer a wide field of investigation to the gynæcologist, and experience daily tells us that physicians outside our special department have the cloudiest ideas as to the site, importance, and diagnosis of these maladies. This defective knowledge is in great part due, on the one hand, to ignorance of the relation of the serous membrane to the genital apparatus and the pelvic cellular tissue, and on the other to ignorance of the relation of the latter to the uterus and vagina. Therefore, at the risk of going over ground with which you are already familiar, I will to-day make two or three remarks upon this relationship; these perhaps will give you a clue as to the etiology of the diseases we are discussing.

If you make an antero-posterior median section of the pelvis you find that the serous membrane is tightly and immovably fixed to the fundus and body of the uterus as far downwards as the middle of the organ; near the so-called isthmus, somewhat above the internal anatomical orifice, the attachment becomes looser and the peritoneum gets so much detached that both anteriorly and posteriorly the subserous tissue becomes distinct. When the bladder is moderately full the peritoneum in front of the uterus turns upwards at a sharp angle on a level with the internal os and makes its way to the summit of the bladder, but the subserous tissue here is so loose and the serous membrane so movable, to correspond with the various conditions of the bladder, that the vesico-uterine pouch may be much deeper at one time than at another, indeed it may disappear altogether if the bladder be very much distended. But in any case we find, lying between the junction of the anterior vaginal wall with the cervix and the anterior fold of peritoneum, a layer of connective tissue—inferiorly denser and provided with muscular fibres which attach the cervix to the bladder, superiorly very loose and meshy, and without any fat—this may be the seat of inflammation and suppuration (ante-uterine phlegmon).

On the posterior uterine wall likewise the peritoneum leaves the muscular tissue on a level with the isthmus; but instead of forthwith running upwards directly alongside the neck, it forms a thick fold rich in loose, fatless cellular tissue and blood-vessels on a level with the upper portion of the neck. From this fold the peritoneum proceeds downwards to cover the posterior wall of the cervix and a portion of the posterior wall of the vagina 1—2 cm. in length, getting closer to these parts as it does so, until it becomes pretty tightly attached to the vaginal wall. Thus the subserous tissue which is most abundant in the upper portion of this fold becomes gradually less so below; in this fold the subserous tissue is especially rich in veins and lymphatic vessels. Where the peritoneum turns upwards posteriorly, at the bottom of Douglas' space, it forms a rather more obtuse angle than it did anteriorly, and the turn takes place about 6 cm. above the anus. Owing to this arrangement of the parts we may get a retro-uterine or retro-cervical phlegmon.

The serous membrane also invests the corpus uteri pretty closely laterally as far as the middle of the organ, though the union is not so tight as it is at the back and the front. At the point mentioned, however, the membrane leaves the womb suddenly and becomes flattened out; in this way the subserous tissue forms a three-cornered mass with apex upwards at the sides of the uterus. The peritoneum then sinks down into the cavity of the pelvis, and having formed the broad ligaments it spreads itself anteriorly and posteriorly over the wall of the small pelvis. Beneath this layer of serous membrane, between it and the levator ani with its fascia (the pelvic diaphragm) lies a large bed of connective tissue, the parenchyma of the broad ligaments. Thus we have two cavities at the sides of the uterus: the upper, the Cavum pelvis peritoneale, forming a portion of the peritoneal cavity, the lower, the Cav. p. subperit. lying between the serous membrane and the pelvic diaphragm and containing the so-called pelvic cellular tissue; below the pelvic diaphragm lies the perinœal region, which only communicates with the subperitoneal cavity by means of small apertures, for it is closely separated from it (Luschka calls this last cavity the Cav. p. subcutaneum). Of these cavities the subperitoneal is the most important pathologically; for besides many large

blood-vessels it also contains venous plexuses and numerous lymphatic glands and sinuses, as well as nervous fibres; the veins and lymphatics being more especially abundant near the uterus and communicating with it; near the uterus also the subserous tissue reaches higher than it does elsewhere, and at the same time joins that at the back and front of the uterus. Thus it appears that the uterine cervix is imbedded in a fatless layer of cellular tissue which becomes gradually more abundant from above downwards. This layer possesses a peculiar interest to us,—

(1) because it is intimately connected with the parenchyma of the cervix uteri and forms a kind of capsule to its adventitia;

(2) because it not only transmits the blood and lymphatic vessels but also contains cavernous tissue (Rouget);

(3) because it readily sympathises with all irritable conditions and swelling of the cervix, and owing to the thinness of the latter is easily lacerated with it, and even if not actually injured easily becomes inflamed after any lesion of the cervix, and offers a very convenient spot for the formation of pus and the transmission of the septic fluid into the blood and lymphatic vessels.

It was specially for the puerperal inflammation of these cellular sheaths surrounding the lower portion of the uterus and upper portion of the vagina that Virchow (Archiv xxiii.) introduced the word Parametritis. But since then this word has been extended to inflammation of the parenchyma of the Lig. lata and even to the whole of the pelvic ligaments (A. Duncan, "On Perimetritis and Parametritis"), and in this way some confusion with regard to these affections has arisen. Pathologically, these different parts of the pelvic cellular tissue vary very much in importance, it is therefore essential that they should have special names applied to them. Consequently that portion of this tissue which forms a layer about 2 cm. in thickness around the lower portion of the uterus and the upper portion of the vagina, and which is so rich in blood-vessels and lymphatics, we shall call the "parametral" tissue, and the inflammatory infiltration and induration of the same, *parametritis*, or what is better, to avoid misunderstanding, "*parametral inflammation*;" but to inflammation of the main portion of the pelvic cellular tissue, *i.e.* to inflammation of that

portion which forms the parenchyma of the Lig. lata and continues thence over the rectum, the iliac fossa and anterior abdominal wall, etc., we apply the term used by the French authors—phlegmon of the broad ligament or cellular tissue of the pelvis.

From the foregoing anatomical sketch it is perfectly plain why the deposit of a mass of exudation anteriorly within the cavity of the pelvic peritoneum—an intraperitoneal exudation—is of the very greatest rarity, whilst its occurrence laterally is almost unknown, and why it almost always takes place in Douglas' pouch behind the uterus and vagina (I am not speaking of those pseudomembranous formations and their expansions upon the serous membrane, for they do not form "tumours"). For at first this peritoneal exudation is fluid and therefore runs to the lowest point; and Douglas' pouch being, in the usual position of the body, the lowest portion of the peritoneum, the fluid will collect there. A tumour will, however, only become apparent when, owing to the gluing together of the intestinal coils and the pelvic peritoneum, the exudation has become encapsuled. I have witnessed this especially well after ovariotomies in which the febrile symptoms indicated pelvic inflammation, but it was only some days afterwards that the exudation-tumour could be made out. Sometimes, however, the encapsuling takes place rapidly, and not a few of the so-called retro-uterine hæmatoceles have been such intraperitoneal retro-uterine exudations. The surface of these tumours is always smooth and their contour pretty symmetrical, because they are formed upon the smooth and symmetrical peritoneal sac. The lowest point of the tumour is always in the middle line; sometimes it is far below the level of the lowest portion of the uterus, for in women who have borne children Douglas' pouch often reaches down much lower. Laterally it never reaches the wall of the true pelvis, but as it gradually approaches it from below it has an ovoid contour. The uterus is always pushed forwards; whether it is pressed directly forwards, or forwards and upwards, depends upon the other relations of the uterus and the depth of Douglas' pouch.

Intra-peritoneal exudations never have any other than a retro-uterine position unless they collect within previously

existing cavities formed by false membranes and adhesions, such as are sometimes to be found between the bladder and the uterus, and sometimes between the uterus, Fallopian tubes and ovaries; but distended coils of intestine in the posterior division of the peritoneal cavity at the side of the uterus may simulate a tumour. The high position of these exudation-tumours, and their constant position at the side of the posterior peritoneal cavity, is a great help to their diagnosis, still there are cases in which their distinction from phlegmonous nodes is exceedingly difficult.

The parametral phlegmons form tumours as soon as they exist, because they are completely enveloped in connective tissue. At first they are doughy or even fluctuating, afterwards firm, hard, knotty, cord-like. They may be found on all sides of the neck. They are least often found in front of it, and when so found they do not reach a large size, because the parametral tissue between the cervix and bladder is most sparingly developed. These ante-cervical phlegmons seldom press down hard upon the vaginal wall, because they encounter much more resistance from the strong fibres which connect the lower portion of the cervix with the bladder than they do from the loose and movable subserous tissue above; they may therefore push high up against the abdominal wall and be felt above the pubes as ante-uterine tumours. Parametral infiltration may also be found behind the cervix and vagina, retro-cervical parametritis not being a rare affection, but it is usually mistaken for an intra-peritoneal exudation, though a combination of the two is sometimes met with. Retro-cervical infiltrations are characterised by their ill-defined diffuse contour below, their gradual extension into the broad ligaments, their marked retro-vaginal position, and by the manner in which they push the cervix forwards and upwards. But parametral exudation-tumours are most frequently found at the side of the cervix where the subserous tissue is the most abundant and affords the best site for inflammatory collections. They embrace the neck closely both anteriorly and posteriorly, and seem as if they have grown out of it. If they are extensive they spread outwards into the broad ligaments—and in this way a phlegmon of the broad ligaments may result from parametral inflammation. Still we do get primary phlegmon

of the broad ligaments as a distinct affection running its own course. These nodes in the broad ligaments vary in size and position; sometimes the tumour will occupy the whole subperitoneal cavity, sometimes it is situated near the cervix, sometimes near the pelvic wall, sometimes it lies more in the posterior portion of the cavity and sometimes it will lie more in the anterior (Exudations under the posterior or anterior layer of the Lig. lat.). They always press downwards distinctly, diminishing the adjacent portion of the vagina, and can only be felt above the pubes when they get large. They are seldom sharply defined, their surface is mostly irregular and knobby, and they gradually merge into the surrounding healthy tissues; as a rule they reach out to the pelvic wall and appear to grow to it at certain points. If the affection has made but little way and there be no parametral mischief the uterus remains unaffected; at another time it may be pushed over to the side opposite to the infiltration, and at another it may be dragged over towards and fixed against the contracted exudation. If the tumour be situated mainly in the anterior portion of the subperitoneal cavity we then feel the mass at various heights in and above the inguinal region like a board with a sharp upper border pressing against the anterior abdominal wall and losing itself gradually towards the iliac fossa. If it be situated in the posterior division of the cavity we can feel it at the side of and behind the cervix tightly attached to the sacrum and surrounding or pressing upon the rectum laterally. In some few cases I have found an isolated exudation-node firmly attached to the lower border of the iliac fossa, with a small irregularly shaped nodule stretching into the outer portion of the Lig. lata (under the serous membrane and on the iliac fascia, and probably derived from the subserous tissue near the Fallopian tubes and ovaries).

I do not intend to go further to-day into the course of these inflammatory conditions. You know how, provided they are not very extensive and do not originate in infection from without, they are re-absorbed and only leave behind them thickenings of the serous membrane, adhesions or pseudomembranous formations, or how, after being encapsuled they may empty themselves into neighbouring cavities. Amongst phlegmonous cases sup-

puration is not uncommon, the accumulation of the pus taking place in one of the larger cavities; but I have only seen the formation of an abscess recognisable as such during life in exceptional cases. The pus may get inspissated or find its way into a neighbouring cavity (most frequently into the rectum or vagina, least so into the cavity of the peritoneum), or it presents itself beneath the skin, though it scarcely ever does so in the perinœum, for the pelvic diaphragm offers such a very strong resistance to it in that direction; usually, from whatever source it may spring, it takes the round ligament as its guide to Poupart's ligament and the inguinal ring and spreads itself thence over the iliac fossa; oftentimes it will rise from this point up into the retro-peritoneal tissue. You will find the best information as to the causes of these different events in König's lecture. The most frequent result of parametral and pelvic phlegmon is thickening and contraction of the cellular tissue, and the formation of the parametral nodes and cicatrices which you have seen so frequently here.

I need scarcely mention that combinations of peritoneal and connective tissue exudations are met with, and especially so behind the cervix; the intimate relation between the pelvic cellular tissue and the serous membrane accounts for this. Though it is difficult under the foregoing circumstances to discover which was the primary affection, I think that the subserous is so in most cases; and this leads me to make a few remarks upon the etiology of these inflammations.

I assigned such great pathological importance to the parametral tissue, on account of its richness in blood-vessels and lymphatics and its intimate connection with the lower portion of the womb and the upper portion of the vagina. And we see why this tissue almost always gets inflamed and swells up after a lesion of the cervix, no matter how it is brought about and whatever its extent. Injuries of the lower portion of the uterus and upper portion of the vagina are the most frequent causes of parametral inflammation, and it is not an accidental coincidence, that no portion of the genital tract is injured so frequently as this, and that there is no gynæcological malady so common as these inflammations. We get, therefore, in the non-puerperal female a state of things which, since Meckel and Virchow's time, has been commonly recognised with regard to puerperal women (*vide*

my lecture upon the nature of puerperal fevers); and there is also no doubt that a very great number of these old inflammatory changes which we meet with as occurring in the parametric and pelvic cellular tissue date from the puerperal state. The injuries which the lower portion of the uterus suffers are of various kinds, *e.g.* its attachments may be torn by the movements of the body, it may be injured during coitus, or it may suffer from our diagnostic and therapeutic measures. After cauterisation of the cervix, after dilatation with laminaria or sponge, after injuries with the point of the sound, after dicisions, incisions and amputations, and after the part has been forcibly dragged down, I have seen fever follow very quickly, and an examination has shown that parametral inflammatory infiltration is the cause of it, and that it is not uncommonly associated with an extravasation of blood into the tissues. I have always told you that the recognised evils and the fatal consequences which follow the handling of the lower portion of the uterus are, as a rule, brought about in this way. For how far the parametral inflammation will extend, whether it will remain circumscribed or lead to a considerable infiltration of the pelvic cellular tissue, whether it will disappear altogether, or leave behind it contraction of the tissues or lead to the formation of an abscess, whether it will develop into an ascending retro-peritoneal phlegmon, or whether peritonitis will make its appearance—that depends upon accidental and individual causes and is out of our hands. There is unfortunately not the slightest doubt, as I have had abundant proof in private practice, that a septic parametral phlegmon and even acute septicœmia may be developed in this manner by infection from an insignificant wound on the cervix in the non-puerperal condition, just as it occurs in the newly-delivered female. Whilst I consider the puerperal state and after this, injuries as the most frequent causes of parametral inflammation, I do not deny their spontaneous origin, their development from so-called internal causes, from spontaneous diseases of the cervix, and from chills. Indeed, the primary phlegmons of the broad ligament—which no doubt are rare compared to those originating in the parametrium—usually begin in this spontaneous manner; starting, for instance, as swellings of the cellular tissue surrounding the hilus ovarii whilst the organ is in an irritable condition, or as small men-

strual extravasations in the broad ligament; and now and then they originate in some external injury.

We now come to the causes of pelvic peritonitis. I have already told you that it may go hand in hand with or follow para-cervical inflammation; sometimes it makes its appearance after the stretching and tearing of long antecedent and slowly-formed false membranes and adhesions; as a rule however it is the result of some affection of the uterus or of the lining membrane of the Fallopian tubes, an endometritis or salpingitis, or it follows some menstrual disturbance. Virulent blennorrhagic inflammation of the uterus and Fallopian tubes may be especially mentioned, as well as that damming-back of small quantities of blood during menstrual disturbances, giving rise to extravasations into the cavity of the peritoneum, and even upon the surface of the outer end of the Fallopian tube and upon the surface of the ovary.

Parametral nodes therefore, as a rule, originate in a diseased condition of the lower portion of the internal genitals, phlegmons of the broad ligament follow those of the parametrium or they arise in the para-ovarian tissue, whilst pelvi-peritonitic exudations accompany affections of the corpus uteri and Fallopian tubes, especially those attacking their inner surfaces. The exudations within the peritoneum are much less common than those in the parametrium and are much more frequently of a secondary nature; they scarcely ever run their course so insidiously as those in the cellular tissue are apt to do. It is only by early and careful investigation that we are able to learn the mode of origin of the disease; frequently we cannot discover it, and, as I have told you before, the history of para- and peri-uterine, etc., inflammations will for a long time afford a wide field for both clinical and anatomical study to the gynœcologist.

# THE ACTION OF DIGITALIS.

BY

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GENTLEMEN,—Those amongst you who during the summer holidays have wandered over the Harz mountains will have noticed the pale red flowers which, especially at the northern foot of the Brocken, sometimes singly, sometimes in groups, attract the attention of the passer-by. If you ascend the lovely Ilsethal, or go from Harzburg over the mountains into Okerthal, you will not infrequently find the woods bedecked with rosy shadows which on a nearer approach resolve themselves into these flowers. Old Leonard Fuchsius, who was a professor at Ingoldstat and Tübingen in the sixteenth century, is said to have been the first who gave them the name of Digitalis; whilst there does not appear to be a single passage in the writings of the ancients which can with any certainty be said to refer to these plants. And yet, owing to their beauty, one can hardly believe that they could have remained undiscovered in former ages, especially as they are common in Southern Europe. We find them moreover in the Alps and in Germany, also elsewhere than on the Harz, sometimes in mountainous districts, sometimes amongst sandy and chalky hills.

The Red Thimble, Digitalis purpurea, or Foxglove, as it is called in England, belongs to the order Scrophularinaceæ; it is a biennial plant from three to four feet high, usually with a single roundish stalk, dark green longish-elliptic leaves and upright, single drooping clusters of bloom, which in July and August develop into bell-shaped, rose-red petals, with beautifully marked eye-like spots and soft inturned hairs.

The leaves more especially interest us, for in Germany they

are the only portion of the plant which is employed medically; but in England the seeds are also sometimes used; and there are some grounds for this, seeing that they contain proportionally a much greater quantity of the so-called Digitalin, which is the essential principle of the plant and no doubt contained in every part of it. As a rule the name Digitalin is understood to apply to a substance discovered by Homolle in 1845; this substance is of a greenish-white colour, uncrystallisable, with a very bitter taste and faint sweet odour, easily soluble in alcohol and slightly so in water and ether. On boiling a solution of it in water and sulphuric acid it splits up into grape sugar and two other organic substances, and for this reason belongs to the chemical family of the Glucocides. But digitalin is not the only active principle in digitalis. In the year 1867 Nativelle got a crystallisable body out of the residue left in the preparation of this substance, and for this the discoverer now claims the name Digitalin: and it appears that he has not yet exhausted the list of essential substances contained in the plant. We do not as yet know in what way the effects of these two bodies differ from one another; but so far as we can judge from the results of exact observations, the physiological effects of the watery extract of digitalis (the form chiefly used in the medical profession) and the physiological effects of Homolle's digitalin are so much alike that we must conclude that the active principle is identical in both.

The theory of the so-called specific action of remedies is intimately connected with the old idea of the entity of diseases. For so long as we look upon diseases as distinctly defined individualities our sole endeavour must be to discover some means for their destruction. If we were to grant that such an annihilation of definite diseases by definite remedies were possible, then the sole aim of practical medicine would be to discover such agents, so that we might pit against each enemy of health and life a mightier foe in the shape of a curative remedy. The art of therapeutics would indeed be a very simple matter if it consisted of nothing more than this. But unfortunately the charming idea has gradually vanished with the discovery that diseases are in reality most complicated disturbances of the normal processes of life, and that in apparently similar cases these disturbances differ widely from one another. In the old

meaning of the term there is no such thing as Specific Action of Medicines. For in the few diseases (ague and syphilis) in which we are able to exert any curative influence by definite remedies, we do not do it so much by a direct destruction of the disease as by a removal of the poison upon which it depends.

But there is really such a thing as specific action of remedies in another sense. When a remedy or a poison—for the two are identical in most cases—gets either directly or indirectly into the blood, it is spread quickly through the whole organism and comes into intimate connection with almost every part. But it does not produce a change in every part of the body, indeed the effect of the remedy is, as a rule, limited to certain sharply-defined regions where it may affect both activity and form, simultaneously or otherwise. In other words: of the endless elementary organisms which compose the animal body there are comparatively few which have definite relations with a definite remedy; upon the remaining portions of the body the remedy has no effect whatever. In this sense we can speak of a definite relationship between certain elementary organisms and certain chemical substances, or what means the same thing, of a specific action of remedies. The only object of scientific therapeutics is to discover these relationships between certain remedies and certain elementary organisms, *i.e.* to localise the effects of our agents. There is no doubt that investigations in this direction, and with a knowledge of this fact, have not been carried on for long, and herein chiefly must be our excuse for the great gaps which our knowledge of the actions of medicines exhibits. Scientific pharmacology is a rich but at the same time an untilled domain, which only here and there exhibits a hopeful and promising green spot on its wide expanse.

With regard to the action of digitalis we know little more than the fact that it possesses the power of affecting the activity of the heart and blood-vessels. For the relation of this drug to other organs either originates in its action upon the heart and vessels, or its effect is so transitory that we can scarcely form any opinion about it, and are still less able to decide in what way the relationship is brought about.

There is no doubt that we can reduce the rapidity of the pulse by digitalis or digitalin. This has been long known as

a general fact with regard to the acceleration of the pulse in febrile subjects. But repeated, exact and scientific observations upon the action of this drug in pneumonia and acute articular rheumatism were first made by Traube; later on Wunderlich, Ferber, and Thomas did the same with regard to typhoid fever, pleurisy, and erysipelas. It also appears from the exact investigations of Stadion and Saunders that the frequency of the pulse in healthy persons is lessened by digitalis. It is said that the pulse always quickens by a few beats for a short time after a small dose, but that after a large dose the stage of acceleration is very short or altogether wanting. If the exhibition of digitalis be continued for a long time in healthy persons the pulse becomes irregular, intermittent and less resistent.

The changes in the rapidity of the pulse are most distinctly seen when we make an experiment with the blood-pressure in a large animal—a dog, for instance—and transfer the pulse-wave to the drum of the kymograph in the usual manner. If in such an experiment a large dose of digitalin or an infusion of digitalis be injected into the jugular vein towards the heart, we see, usually whilst the injection is taking place or at least immediately afterwards, a very considerable diminution in the rapidity of the pulse. This takes place in connection with diastole alone. No matter how many curves we compare we always find, even during the greatest retardation we can produce by the digitalin, that the duration of systole is unchanged; and this is the case even when, as is very commonly the case after the injection of digitalin, the pulse-curves have run up to a great height. The diastole on the contrary may be lengthened out fourfold or even more.

If we divide the diastolic curve into three equal portions we shall then find, as a rule, that the speed is least in the middle, greater in the last, and greatest in the first portions.

As you know, Ed. Weber was the first to prove that the frequency of the cardiac contractions diminishes when the vagus is irritated mechanically, chemically, or electrically, and that if the irritation be strong the heart ceases to beat during diastole. In man and mammals during the whole life the vagus is under active stimulation from its roots in the medulla oblongata. For if the vagus be cut through the frequency of the heart's action is suddenly increased.

Traube, making use of Weber's experiment, also attributes the constant retardation of the pulse after digitalis to stimulation of the controlling-nerve of the heart, and therefore to a process similar to that which we can bring about by Faradisation of the vagus. It also appears, as Traube himself states, that after the introduction of digitalis into the system the rapidity of the pulse lessens even though the vagi have been cut through and in fact separated from their centres. Traube therefore feels bound to conclude that the retardation of the pulse which makes its appearance subsequent to the division of the vagi is due to the action of the digitalin upon the peripheral segment of the nerve. But still there is no direct proof from these facts alone that the vagi are connected with the retardation at all. The objection might still be raised that perhaps the pulse lessened in frequency from some other cause wholly unknown and altogether unconnected with the vagi; and this objection would be valid so long as we were unable to exclude the influence of the vagi during the action of the digitalin. In atropine we possess an agent by which we can entirely neutralise the influence of the vagi, as was shown by Bezold and Blöbaum about five years ago. If a sufficient quantity of this material be injected into a dog the activity of the vagi completely disappears, and then if digitalin be injected there is not the slightest retardation of the pulse. It is thus shown that when the influence of the vagi is excluded digitalin loses its power of retarding the pulse—we may therefore safely conclude that the effect is brought about by the cardiac vagus.

Contrasting with this retardation of the pulse and corresponding exactly to what follows when the vagi have been divided, we notice that after the pulse has been going slowly for some time it suddenly becomes considerably quickened. Traube, drawing his analogy from Weber's experiments, attributes this quickening to a paralysis of the controlling-fibres of the cardiac vagus. And he is quite correct. For during this acceleration we may transmit the strongest induction current down the vagi without in the slightest degree lessening the rapidity of the heart's action. But I feel quite certain that this acceleration of the pulse after digitalin is not solely and entirely due to a paralysis of the controlling-fibres of the

cardiac vagus, but that a direct stimulation comes into play. By means of atropine we can, as I showed before, paralyse the cardiac vagus. A consequence of this paralysis is a considerable acceleration of the pulse, just like what occurs after the vagi have been divided. Now, when digitalin is injected into a dog under the influence of atropine there is not only no retardation of the pulse, but on the contrary it becomes much more rapid.* This could not have occurred if the accelerating effects of digitalis were limited to a paralysis of the vagus, for the paralysis had been completely effected by the atropine. We are thus driven to the conclusion that digitalis not only gives rise to an irritation and subsequent paralysis of the controlling-fibres of the heart, but that it also irritates the motor nerves; a fact which appears to be analogous to an observation of Rutherford which is mentioned by Schmiedeberg,† viz. that an increased action of the heart may be brought about in a dog under the influence of atropine—and thus with its controlling-fibres paralysed—by peripheral irritation of the vagus.

During this stage of acceleration of the pulse a sudden and permanent stoppage of the heart may occur and the animal may immediately die. Far more frequently, however, another change in the pulse occurs, during which, moreover, the irritability of the vagus is completely lost. This change consists chiefly in a great irregularity, usually combined with marked retardation. Sometimes the curve will rise slowly and steadily upwards and as slowly fall either before or after it has reached its former height; sometimes the rate of the rise will differ from the rate of the fall; or perhaps isolated, small, quick beats will make their appearance at the end of systole; at another time, and not uncommonly too, the curve rises and falls broken by little interruptions equal in length to the small, quick pulsations which appear at the end of the systole. The whole gives one the impression that there must be two powers at work in the heart at the same time, one of which gives rise to the

* A dog under the influence of atropine had a pulse of 14 for every six seconds, digitalin was injected, there was no diminution whatever, but the pace gradually increased to 16, 21 and 22 beats per six seconds.

† Ueber die Innervationsverhältnisse des Hundeherzens. Arbeiten aus der physiol. Anst. zu Leipzig. Sechster Jahrgang, s. 37.

larger and the other to the smaller contractions. Possibly the ventricle may give rise to the former and the auricle to the latter. If the larger contractions follow one another so quickly that they exceed the smaller in rapidity, the latter are not to be seen at all, whilst, on the contrary, the smaller become more and more evident the more slowly the larger make their appearance, and moreover are to be seen in their full development when there is a complete pause in the larger. It is impossible at present to give an altogether satisfactory explanation of this behaviour of the pulse, though it is not uncommonly met with. We call it want of rhythm, and Heidenheim has conjectured that in such cases we have to do with an irritation of the intra-cardial controlling-apparatus. But still the want of rhythm makes its appearance when the cardiac vagus has been previously paralysed with atropine. Nevertheless, Heidenheim is of opinion that every portion of the intra-cardial controlling-apparatus is not paralysed by atropine. He says, it is very probable that the fibres of the vagus in the heart are joined first of all with ganglion-cells, and that from them go forth the fibres of junction with the true cardiac motor-ganglia. In Heidenheim's opinion there is nothing contradictory in the observations which have been made with regard to the effects of atropine if we limit its paralysing power to the vagus-ganglia; when the fibres which join these vagus-ganglia with the true motor-ganglia are irritated they act upon the latter ganglia in the same checking manner as the ganglia of the checking apparatus itself. But certain actions, or more correctly the absence of certain actions of digitalin upon the heart, seem opposed to this view. There cannot, for instance, be the slightest doubt that the retardation of the pulse which makes its appearance immediately after the injection of digitalin is due to an irritation of the vagus. But this retardation of the pulse after digitalin does not make its appearance at all when the animal has been put under the influence of atropine. Nevertheless we should expect it to do so if, as Heidenheim supposes, the fibres which go from the vagus-ganglia to the motor- heart-ganglia did not lose their irritability under atropine.

Be this as it may, however, we must always look upon the want of rhythm, if it make its appearance after digitalin, as a threatening symptom; and amongst the very numerous experi-

ments I have made upon animals with regard to the effect of this drug, I have never seen a single case in which this want of rhythm gave place to a regular action of the heart before death.

The final stoppage of the heart, the special cause of death in poisoning by digitalin, is due to a great loss or complete destruction of sensibility in the muscles of the heart. Immediately after a deadly dose of digitalin we may pass the strongest current through the heart; we shall either see no effect at all, or at most only a very limited contraction. Whether we have in this case a paralysis of the nervous or muscular elements of the heart we know not. Rud. Böhm considers the latter is the case, at least in the frog's heart.

Besides the above described effects of digitalin upon the pulse, there is another well marked phenomenon which makes its appearance in connection with the circulation, but which cannot be accurately observed unless we employ the kymograph. This consists in a very great increase of arterial pressure after the exhibition of large doses of digitalin. As a rule this effect makes its appearance at the same time as the change in the pulse; we see accordingly that the pressure-curve begins to rise immediately after, or even during the injection of the digitalin, whilst at the same time the rapidity of the pulse diminishes, but in very exceptional cases there is a diminution of pressure immediately after the injection and whilst the pulse is being retarded, but it always becomes reversed after a very short time. Little by little, sometimes quickly, sometimes slowly, but almost always without a break, the pressure goes on rising to a very considerable height, to such a height indeed that it not infrequently exceeds 300 mm. of mercury, even in a small or medium sized dog. Then it sinks down again slowly, and remaining usually for a time below the normal, sinks at last, when the heart ceases to beat, in a sloping line almost down to zero. It is specially worthy of notice that these variations of pressure make their appearance quite independently of the greatest variations in the rapidity of the pulse or height of any special pulse-waves, in such a manner that the most varied arterial pressure may be combined just as well with a slow and very undulatory as with a quick and little undulatory pulse. This would appear to indicate that

these variations of arterial pressure which make their appearance after the exhibition of digitalin are independent of cardiac activity, and leads us in the next place to inquire as to the origin of this increase of arterial pressure which we see making its appearance after the injection of digitalin.

If we inject a solution of digitalin into the jugular vein of a rabbit after we have laid open the abdomen and brought the mesentery into view, we can easily see what a wide-spread alteration takes place in the size of the vessels some time after the injection. The changes may even be so great as to produce complete closure of some of the vessels. For we can see that some of the red streaks are not only reduced in size, but sometimes they disappear altogether. In these experiments it is well to keep up artificial respiration, for large doses of digitalin not infrequently give rise to strong convulsions in the rabbit, so that the breathing becomes laboured and even interrupted, and carbonic acid accumulates in the blood, which, you know, will of itself give rise to a contraction of the arteries. But still we see the change in the arteries of the rabbit's mesentery even when artificial respiration is kept up with a pair of bellows, and moreover when the animal has been rendered passive by means of curare; so that we can no longer doubt that these changes in the mesenteric arteries are due to the action of the digitalin. The question then arises, by what means does digitalin bring about these changes in the arteries? On physiological grounds we are led to believe that they are due to an irritation of the vaso-motor centres in the medulla oblongata. For we know several circumstances under which this centrum will cause contraction of the arteries? I may mention $CO^2$, the irritation of a sensory nerve and the direct irritation of the centrum by mechanical or electrical means. Also there are communications from Traube and R. Böhm, which seem to show that the arterial contractions after digitalin are brought about by irritation of the vaso-motor centre. My observations are, however, not in accordance with this. Repeated experiments have convinced me that the changes in the arteries of the rabbit's mesentery make their appearance even when, previously to the injection, the spinal cord is divided on a level with the epistropheus, and is then cut off from all connection with the vaso-motor centre.

It is a well-known fact that arterial pressure in all the larger arteries is raised by a contraction of a great number of arteries, and even by the contraction of a small number simultaneously, and we may therefore conclude that the increase of pressure in the arterial system which follows the administration of digitalin, as it does at least for a time, is due to this contraction. Traube and Böhm have already expressed such an opinion. But they have attributed the contraction and the increase of pressure in connection with it to an irritation of the vaso-motor centre, brought about by the digitalin. For they found there was no increase of pressure when they had previously divided the spinal cord between the atlas and epistropheus. The repeated experiments I have made in connection with this question have yielded results in striking opposition to those of the above-mentioned observers. I found that when I divided the cervical cord on a level with or even a little higher than the epistropheus and then injected digitalin, there was in every case without exception a rise in pressure, and sometimes even a very considerable one. This fact is related to the observation which I have already mentioned to you, viz. that the arteries in the mesentery of the rabbit contracted after the administration of digitalin, even when the medulla oblongata had been previously divided. I have not the slightest doubt that the increase of pressure which makes its appearance in the aortic system after the injection of digitalin is due to a contraction of the smaller arteries; this contraction, however, must not be attributed to an irritation of the vaso-motor centres, but to a direct action of the poison upon the peripheral nerves of the vessels, and possibly even upon the terminal organs in the muscular structure of the vessels.

But is this contraction of the small arteries the sole cause of the increase of pressure? It appears to me that we must at once deny it if the question be taken quite literally. And for this reason, that a rise of pressure in the arterial circulation due merely to impediments or stoppages in the peripheral outflow, without the co-operation of the heart, appears impossible. For the pressure upon the left heart rises as the damming back of the blood in the aorta by the contraction of the small arteries increases; the quantity of blood which reaches the aorta during systole will diminish in the same proportion, and there will,

consequently, be no change of equilibrium. Now there is no doubt that certain relationships between the action of the heart and the blood-pressure, brought about by the nervous system, are already familiar to us. For example, the investigations of Ludwig and Thiry have taught us that increased pressure in the heart and arterial vessels produces an increased rapidity of the pulse. But without mentioning that mere numerical increase of the pulse does not show that there is an increase of power in the heart, Ludwig and Thiry's well-established fact would not be available as an explanation of the rise of pressure after digitalin, because after the employment of this poison we often get simultaneously a rise of pressure and a retardation of the pulse. The simplest explanation is that the pressure of the blood within the aorta is independent of the contractile activity of the heart. In that case with low blood-pressure some of the left heart's contractile activity would have no effect upon the movement of the blood, whereas when the pressure was increased a greater portion or even the whole of the systolic action would come into play to move the blood. The above-mentioned observation, that after digitalin we may have very different arterial pressures simultaneously with the same rate of pulse, and the converse, seems to support this view.

But Böhm has already shown, at least as regards the frog's heart, that after digitalin cardiac power, as such, increases quite independently of the blood-pressure; we may therefore naturally conclude that there will be the greatest increase of arterial pressure when an increase of power in the heart and a contraction of the small arteries occur simultaneously. Still we must admit that the only possible cause of an increase of arterial pressure is a contraction of the small arteries, presupposing only that cardiac activity is not lessened by a rise in pressure.

Even as early as the year 1866 Mantegazza had found that in rabbits and hens the temperature of the rectum fell when the animal was under pain. Somewhat later O. Naumann, independently of Mantegazza, arrived at similar results in his observations upon the human subject, and Dumarquai, during the last war, noticed that the temperature immediately fell, it might be several degrees, after severe wounds complicated with injuries of bone. Lastly, Heidenheim, without knowing of these observations, discovered the important fact that when we

bring about an increased aortic pressure by irritating a sensory nerve, or by directly irritating the medulla oblongata, there is at the same time a fall of temperature in the interior of the body, that is to say in the lower venous sinuses. Heidenheim thinks that this fall of temperature is due to an increased escape of caloric, this again being due to an acceleration of the circulation following upon the rise of pressure in the arteries. If, for instance, owing to an increased rapidity of the circulation a larger quantity of blood were carried in a given time from the interior warmer parts of the body to its cooler periphery, it is possible that the temperature of the whole of the blood might fall, owing to the contact of so large a quantity with a cooler medium. Several experiments which Heidenheim has performed lend support to his theory. He first of all proved that when we increase the arterial pressure by the irritation of a sensory nerve, or by the direct irritation of the vaso-motor centres, we get an increased pace of circulation in the arteries, the crural veins, and the external jugulars. When, on the contrary, he reduced the swiftness of the current by section of the vagus or by sudden and copious bleeding, he observed that there was a rise of temperature in the interior of the body. As a proof of the dependency of this rise and fall of temperature in the interior of the body upon the warmth of the periphery, he showed that the acceleration of the circulation caused more or less fall of temperature in the interior of the body according as the surface of the body was kept cooler or warmer. But the observation which he has only just published appears to me to give the greatest support to his theory. In this he proved directly, by means of simultaneous determinations of temperature on the surface and in the interior of the body, that when arterial pressure was increased by irritating a sensory nerve or the med. oblong., the temperature of the skin rose whilst at the same time that of the interior of the body fell, and that on the contrary after the withdrawal of the irritation the temperature of the skin sank, whilst that of the interior of the body rose, when the amount of pressure showed that the rate of the circulation had become reversed. These experiments of Heidenheim I have repeated many times, but with this difference, that I did not bring about the rise of pressure by direct or indirect irrita-

e vaso-motor centre, but by injection of digitalin into

the blood. My results correspond entirely with those of Heidenheim, and I have therefore not the slightest hesitation in concluding that the decrease of temperature within the body after digitalin is caused by an acceleration of the circulation in the peripheral portions of the body. An irritation of the vasomotor centre is not an essential element in the production of this increase of pressure by digitalin, for, as I have previously stated, the rise after the exhibition of this poison takes place even when the cord has been previously divided. But there is no doubt that digitalin produces its effects by diminishing the size of the small arteries and impeding the flow of blood through them; so that really there is but one cause for the rise of arterial pressure, whether it be due to direct or indirect irritation of the med. obl. or digitalin, and that is contraction of the small arteries.

The next question that arises is whether a universal acceleration of the circulation is compatible with a contraction of the smaller arteries. I think it is not, if contraction take place in all or a very great number of arteries, but that it is, if contraction be confined to a limited area. For instance, a moment's consideration of the narrowed, or it may be completely closed arteries in the mesentery of the rabbit will convince us that the circulation through them could not be accelerated. But if we suppose that this condition is not a general one throughout the body, and that the arteries in their smaller ramifications in many other parts are not narrowed, we shall get compensatory hyperæmia. The blood-pressure in the whole aortic system must increase, and at the same time there will be a wider difference of pressure between the arteries and veins. But the circulation will be accelerated in those parts only where the blood can run freely from the arteries to the veins. Now if we imagine that digitalin acts only upon the arteries of the interior of the body, and that during its action those leading to the surface retain their usual direction, we can easily explain the rise of temperature in the skin and the fall of temperature within the body. Heidenheim seems to hold this view according to his latest publication.

Hitherto we have been considering the so-called physiological effects of digitalin, and the knowledge we have gained with regard to them is the result of a strictly scientific method.

We have now to inquire whether, and if so, to what extent, our knowledge of the effects of digitalin on the healthy organism helps us in the explanation of that empirically discovered power which our agent exercises over morbid changes in the organism, or as we are accustomed to say, how it explains the therapeutic effects.

We employed digitalis in the so-called organic diseases of the heart before we had any very clear idea what the changes in the circulation were in these affections. As a rule, we were contented with the theory that digitalis reduced the "strength and rapidity" of the circulation, and considered that this quite explained its effects, not only in diseases of the heart and blood-vessels, but also in hæmorrhages and fevers.

To Traube belongs the credit of having been the first to trace the power which digitalis exercises over those difficulties in the circulation due to heart disease to simple mechanical circumstances. The effect of the drug in these conditions consists chiefly in its power of removing the persistent general venous hyperæmia. And as this venous congestion is the main cause of the numerous dangers and difficulties in heart diseases, we can see how it is that digitalin has such a marked influencc over these affections. But this by no means applies to all diseases of the heart, it is more particularly the case with insufficiency of the mitral valve and stenosis of the left venous ostium. In these cases, so long as the increased burden which is thrown upon the different portions of the heart is compensated by hypertrophy, we get but little interruption of the circulation. But as soon as the muscular tissue of the heart ceases to hypertrophy in proportion to the increase of pressure, the quantity of blood which will reach the aorta during each systole will diminish, and, consequently, we shall get a general deficiency of blood in the arteries, and following upon this last again, as its sequæla, we shall get congestion of the venous system, with its accompanying catarrhs, transudations, etc. All this quickly changes after a few doses of digitalis, and it is not impossible that it does so wholly in consequence of the effect of our remedy upon the small arteries. For as soon as the latter contract in different parts of the circulation, the current of the [illegible] must naturally become impeded. In [illegible] ter quantity of blood

will accumulate in the arteries, the difference of pressure between the arteries and veins will increase in favour of the former, and the blood will rush through the capillaries at a greater pace, and, owing to the pressure with [illegible] the dropsical effusions will [illegible] the same time the watery [illegible] and cause great diuresis. [illegible]

Finally, our remedy plays a very important part in the treatment of that weak condition of the heart which was first described by Stokes under the name of "weakened heart," and which is now usually attributed to granular and fatty changes in the substance of the heart by German physicians. The lessening of the heart's energy, which is always present in this disease, besides leading to several other symptoms into the consideration of which we cannot now enter, also often leads to impediments of compensation in the circulation which may have the greatest similarity in their consequences to the impediments of compensation which occur in organic cardiac diseases, and also agree with them in this, that they present a well marked object for treatment with digitalis. It is surprising how, after a few small doses of digitalis, the heart's action will become regular, the oppression cease, the cyanosis and dropsy diminish, and the urine increase. Traube is of opinion that this affection is due to a kind of paralytic weakness of the nervous check-system, which, by means of the increase in the frequency of the pulse which it induces, brings about a malnutrition of the heart and its consequent fatty degeneration; and he states that in the further course of the mischief nothing more can be done with our remedy, because the check-nervous system becomes insensible to its influence. This may or may not be, but we may be pretty sure that when digitalis is employed in these conditions it acts partly by the effect it produces upon the peripheral arteries. As Traube points out, we may for another reason conclude that this is the case, *i.e.* because a patient will feel relieved when the pulse is not markedly retarded.

Finally, I must here tell you that digitalis has been very often styled a diuretic. But it is only so in a very limited sense, namely, only so far as it gives rise in these dropsical states dependent upon cardiac mischief to a re-absorption of the transudation, and thereby to a diluted condition of blood and an indirect increase of urine. Possibly, also, a direct increase may take place owing to the increased arterial pressure.

Traube also assigns a salutary effect to the retardation of the pulse after digitalin, in so far as the nutrition of the heart is brought under healthier conditions during its slower action, for it [illegible]ing its [illegible]ment mainly during diastole.

Our knowledge of the effects of digitalis in the febrile state is limited to two, no doubt very important, symptoms of fever —the increased pace of the pulse and the high temperature. Our drug, when given in the right quantity and form, possesses the power of lessening both, and even for a time of removing them. But it is not possible to do this for a continuance, or what is the same thing, we cannot cure the fever.

Digitalis, therefore, possesses no power to remove the deeper, and as good as unknown causes of fever; all it can do is to produce a series of temporary changes, which possibly are quite unconnected with those deeper processes, owing to which the rapidity of the pulse diminishes and the temperature falls.

It was formerly thought that a slow pulse indicated a slow circulation, and it was this idea mainly that brought digitalis into connection with the strength and rapidity of the circulation. But experimental investigations showed, what was evident from a simple consideration of the facts, that there is no such connection between the rate of the pulse and the rapidity of the circulation; and since then the value of the mere pace of the pulse in estimating the activity of the circulation has become much lessened. Indeed, modifications in the circulation due entirely to changes in the rapidity of the pulse are as good as unknown. We are, therefore, not justified in attributing the favourable results which follow the administration of digitalis to simple diminution of a morbidly accelerated pulse. It is more than probable that the favourable influence is to be sought for, not in the diminution of the frequency as such, but in the greatly increased results of the heart's contractions, which go almost hand in hand with this diminution. In general, for instance, during the action of digitalis the number of pulse-waves is in inverse proportion to their height, so that the quantity of blood which reaches the aorta during each systole will vary as the rapidity of the pulse. Nevertheless, we do not know whether the total quantity of blood which reaches the aorta in a given time is greater during the frequent but weak contractions of the heart, or during the slower and stronger. Pathological facts indeed appear to point to the latter. But we are not sufficiently well acquainted with their bearings to draw any valid conclusions.

The results of numerous exact observations upon the effects

of digitalis in acute febrile diseases has proved that there is no real connection between a fall in the rapidity of the pulse and a fall of temperature. Sometimes the pulse falls first, sometimes the temperature, and the greatest fall in these two febrile factors by no means always occurs at the same moment, and very commonly the retardation of the pulse lasts longer than the reduction of temperature. Judging from the above mentioned experiments there can be no doubt that the retardation of the pulse which appears after the administration of digitalis during fever is, at least in part, due to an irritation of the vagus. Whether it is entirely so is doubtful, and indeed appears unlikely when we recollect that there is often a very well marked want of cardiac rhythm in the febrile state, similar to that which makes its appearance in animals after a long employment of digitalis. The causes of this "arhythm" are, as we have said, in some cases unexplainable. But it must always be looked upon as a dangerous symptom, for, as we have seen, it is the usual immediate forerunner of the final paralysis of the heart in animals. This fact alone ought to make us pause and consider whether this drug is fit for common employment as an antipyretic. A part of the dangers of a high temperature are to be found in its debilitating effect upon the heart. And it is questionable whether the purely temporary advantages attainable from digitalis are not outweighed by its weakening effect upon the cardiac activity.

My experiments upon the physiological effects of digitalin, as well as the important investigations of Heidenheim as to the influence of the vaso-motor nervous system upon the circulation and temperature, have led to the conclusion that the fall of temperature which takes place after the introduction of digitalis into the circulation has its real foundation in an increased flow of blood to the external integument and peripheral portions of the body. Consequently the results of an injection of digitalin would appear to stand in the same category with the cooling effects which follow the application of a mustard poultice or other cutaneous irritants (O. Naumann), or which follow burns (Fr. Falk) or the application of varnish to the cutaneous surface (Laschkewitsch). In all these cases the result is brought about by dilatation of the cutaneous vessels, whereby a marked cooling of the blood takes place on

the surface of the body. J. Rosenthal has lately made use of this fact as to the cooling of the cutaneous surface in his theory of catching cold. He found that when animals, the temperature of which had been raised by hot air, were transferred to the ordinary atmosphere their temperature fell, not merely to the normal, but considerably below it.

A similar thing, as Rosenthal points out, may occur with the human subject. When we pass from a hot into a cool surrounding medium, the heated blood in the dilated cutaneous vessels becomes suddenly cooled, and may possibly, owing to its low temperature in the interior of the body, give rise to diseases of the internal organs. But I must add that the cooling of the blood at the surface goes on more quickly when the high temperature to which we are exposed makes us long for a cooler surrounding medium. But if it be desired to bring about a well marked cooling of the interior of the body by hyperæmia of the cutaneous surface, it is essentially necessary that this hyperæmia occupy a large portion of this surface.

At first sight we might think it impossible to explain the cooling effect of a mustard plaister or a small burn in this manner, and this applies more especially to the mustard plaister, because the spot upon which it is applied is necessarily covered over and prevented losing a quantity of heat. Nevertheless I believe the effect is brought about in the foregoing manner. For I am of opinion that when the temperature falls after the application of these irritants it does so mainly in consequence of a very large area of skin being cooled, and not that only over which the irritant has been applied; I think the cooling may go on even over the whole cutaneous surface. After the application of a mustard poultice to the skin, we feel an increased feeling of warmth over the whole cutaneous surface. We feel a warm glow all over the body. Again, if the warm hand be laid upon the cold skin of the thigh whilst one is in bed, almost simultaneously the leg and foot of the same side feel warmer, and can be shown to be so by placing the thermometer between the toes.

Notwithstanding that my experiments upon animals yielded strong evidence that the fall of temperature, which takes place simultaneously with an increase of arterial pressure, is due to an increased flow of blood to the integument, still, with the aid

of our present facts only, it would be venturesome to attribute the cooling effect of digitalis in febrile diseases to the same process without further investigation. That such an antipyretic effect follows the employment of digitalis is proved by numberless clinical observations. Nevertheless I do not think we ought to explain it in the foregoing manner so long as a series of preliminary questions remain so unsatisfactorily answered. Heidenheim, by investigations which are of extreme importance with regard to the theory of fever, though they have not yet received much consideration from pathologists, has indicated the direction in which the inquiry should be pursued. He found that in febrile animals, just as in man, there occurred two conditions differing in many respects from one another; these he designated by the long familiar terms—"Asthenia," and "Sthenia." For instance, he has met with febrile animals in which a high internal temperature was joined with a relatively low cutaneous temperature. In such animals the blood-pressure was slight and the energy of the heart had suffered. In another series of cases he found a high internal temperature accompanied by nearly as high a cutaneous temperature. In such animals the blood-pressure was high and the heart capable of vigorous action. To the condition of the animals belonging to the first group Heidenheim applies the term "Asthenia," to that of the animals belonging to the second, "Sthenia." We can explain the antipyretic effect of digitalis upon animals belonging to the first group by reference to its effect upon the circulation and temperature of the body in healthy animals. But its antipyretic effect upon animals belonging to the second group of "sthenic" fevers scarcely admits of such an explanation. For where the temperatures of the skin and interior of the body differ but little or not at all, we can hardly expect a cooling effect from a more vigorous flow of blood at the periphery. We might therefore be inclined to say that the antipyretic effects of digitalis are limited to "asthenic" fevers. But this objection is inadmissible. For we find experimentally that digitalis almost invariably acts as a depressant upon febrile temperature, provided it be given in sufficient doses, the number of exceptions to this rule being so small that there is every probability the depressing effect does take place in sthenic fevers.

But there are some other facts which make us doubt whether we are justified in asserting that the fall of temperature which occurs after the employment of digitalis in *disease* is due to the same cause as that which occurs, simultaneously with a rise of blood-pressure, after the injection of the same drug into the *healthy* animal. Traube says, and I can vouch for it, that nothing is easier than to convince oneself that digitalis in large doses with febrile patients soon reduces the pace of the pulse considerably, and *lessens* the tension of the aortic system to an important extent. In febrile patients again, after the employment of digitalis we often observe a simultaneous fall of temperature and retardation of the pulse. We may therefore assume that a *diminution* of tension in the aortic system may sometimes coincide with a *fall* of temperature in these morbid conditions. Indeed we have direct clinical evidence that the antipyretic effects of digitalis often coincide with symptoms of collapse or asthenia, *i.e.* with coldness of the extremities and trunk, accompanied by livid or yellow pallor, a small pulse, and irregular, hurried, or very shallow respirations.

In addition to this the fall of temperature, which makes its appearance in the interior of the bodies of animals simultaneously with the rise of arterial pressure after the injection of digitalin, does not continue for long, indeed it usually ceases whilst the blood-pressure still continues to rise, and the temperature may afterwards rise higher than it was previous to the increase of pressure, although the arterial pressure remains higher than natural.

On all these grounds it seems advisable to consider that the relationship between the effects of digitalis on the circulation and its effects on the temperature in *fever* is still an open question. Perhaps simultaneous and exact observations upon the tension in the aortic system, the temperature in the rectum and the temperature of the cutaneous surface during the action of digitalis on febrile human beings, will explain it in the same sense as the connection between the fall of temperature, and the increase of arterial pressure in healthy animals has been already pretty well proved experimentally.

We will now, in conclusion, refer shortly to the effects of digitalis upon the functions of other organs besides the heart

and blood-vessels. A few words will suffice. For, as I remarked at the commencement of this lecture, the relation of our drug to other organs is only developed through the heart and vessels, or it appears in a very undecided and intractable shape for physiological demonstration.

Amongst the most troublesome effects of digitalis are its interruptions of digestion, its secondary effects as they are called. Want of appetite, nausea and sickness appear almost constantly after the continued employment of the drug, and not unfrequently they coincide with or pretty soon follow a change of temperature and pulse. The cause of this effect is by no means evident. Still it may be remarked that after other remedies, *e.g.* veretrum, tartar emetic, and saltpetre, by means of which the temperature and pulse can be lowered, similar disturbances of the digestive organs have been observed. If in addition to this well known effect of digitalis we bear in mind the danger there is in its employment if there is any cardiac debility, and also that its antipyretic action never lasts very long, we feel very much inclined to abstain from the use of it in febrile diseases altogether, or to limit its employment to those cases in which we feel certain there is no muscular weakness of the heart.

In the so-called organic diseases of the heart it is, on the contrary, quite indispensable. When it is prescribed under the above-mentioned conditions its effects are often surprising. We may also notice that those digestive disturbances which so frequently make their appearance when this remedy is employed in the febrile state are not only totally absent, but they may even become reversed in such a manner that the tongue cleans and the appetite, hitherto feeble, is regained.

If the digitalis be given for an exceedingly long time or in very large doses, severe nervous symptoms may make their appearance, such as numbness, defective sight, humming noises in the head, faintings, convulsions, coma, and death. A physiological reason for these symptoms has not yet been given. In conclusion, I may mention that a partly erysepelatous and partly papular eruption was set up over the face and trunk many times after the employment of this drug; whether it was really such remains to be proved.

ON

# NEURITIS IN RELATION TO ITS DIAGNOSIS AND PATHOLOGY.

BY

PROF. H. NOTHNAGEL,

JENA.*

GENTLEMEN,—I conducted you a few days ago to the bed of a strong man 19 years of age, who presented all the phenomena of a severe tetanus. The rigid contraction of the muscles of the jaw and trunk, the occasional increase of this tonic rigidity on the application of external irritation, or even apparently without it, made the diagnosis clear at the first glance, apart from all other symptoms. I indicated to you that, basing on certain principles derived from experience, we had every reason to pronounce a very serious prognosis, in spite of the relatively infrequent tetanic spasms, and in spite of the slight increase of reflex irritability. That is to say, the tetanus appeared only a few days (8) after an injury; from the outset the muscles of deglutition were involved in a high degree; then there was a high pulse-rate (120), while the temperature was only slightly elevated (101·7°); finally, and most important, the pectorals and scalene muscles were in a state of tonic contraction, so that the patient could only breathe with his diaphragm, and the mobility of the upper part of the thorax was quite in abeyance. Each of these is, according to experience, to be looked upon as rendering the prognosis more or less serious; and this was

* Compiled from some lectures held in the clinique. Neuritis optica is here put entirely out of consideration.

confirmed in our case, for in spite of energetic treatment (chloral, bromide of potassium, galvanism) the patient died asphyxiated with prolonged coma on the sixth day of the disease.

But it is not to the entire clinical phenomena of tetanus that I desire to call your attention in connection with this case; it is rather a single point deduced from the case which we shall to-day subject to a more thorough discussion—this point has relation to the development of the tetanus in our patient.

The case was one of traumatic tetanus. On the 20th February the patient injured the last phalanx of his left thumb with an axe. The wound at first bled freely, but it appeared so trivial and gave him so little annoyance, that the patient was able to continue his occupation of a cartwright without interruption. On the 27th of February he was exposed, after he had been sweating profusely at his work, to a violent draught. Yet after this exposure to cold he suffered no inconvenience whatever, and there was not as yet the slightest pain in the injured thumb. Next day appeared the first symptoms of tetanus, these rapidly increased, and occasioned the removal of the patient to the hospital on the 29th February.

On clinical investigation, the day following, we found on the last phalanx of the left thumb an elongated cut about 3 centimetres ($1\frac{1}{8}$ inch) long with smooth edges; it began at the inner edge of the dorsal surface of the thumb, where it divided a portion of the nail, and thence passed round to the volar surface. This small wound was in its deeper parts already united by first intention, and had a thoroughly healthy appearance; there was no redness around it, and hardly any swelling. Pressure on it was not the least painful, and produced no tetanic spasms, at least not more than pressure on other parts of the body.

As, according to the direction of the wound, branches of two nerves, the median and radial, must have been encountered, we examined these nerves in their course along the arm. We found as follows:—

Inspection of the arm shows nothing abnormal. The patient also does not spontaneously complain of any subjective sensations or pains. Pressure on the ball of the thumb and the lower part of the forearm is nowhere painful. On the other hand,

there is a spot 2 to 3 centimetres long about the middle of the internal aspect of the forearm, where pressure directed towards the interosseous space is decidedly painful (as compared with the corresponding spot on the right side); likewise pain is produced by moderate pressure on the median nerve at the hollow of the elbow, the pain being felt at the point of pressure itself, and simultaneously stinging pains in the injured left thumb; finally, there results from pressure on the median in the upper arm (which nowhere appears thickened), not pain of any kind, but contractions in the thumb;—all these abnormal phenomena are absent on investigating the right median nerve.

Further, it appears that pressure on the radial side of the forearm, directly over the styloid process of the radius, produces pain, and at the same time slight contractions of the thumb. Then again there is a painful spot on the outer aspect of the upper arm where the radial nerve turns backwards; here again these phenomena are absent on the right side.

On testing by slight pricking with needles, the sensitiveness of the skin appears the same in the two hands and arms; on the other hand the pricking is more acutely felt on the left side of the neck than the right.

When the positive pole of a galvanic current is placed on the inner aspect of the forearm, and the negative pole in the axilla or in the supra-clavicular fossa, the patient feels, when only eight elements are used, acute burning pains on the left side, and when the current is changed contractions follow; on the right side these occur only when the current is much stronger.

You understand, Gentlemen, that in the case of a tetanic patient we cannot make such investigations repeatedly, because even with the greatest care the investigation itself involves sensory irritation. In our case it could be carried out with some degree of completeness, only because reflex irritability was comparatively little increased. Yet this one trial has furnished, as you were able to prove, very definite results.

The group of symptoms which I have just given you, lead us with some probability to the conclusion, that in the left median and radial nerves there exists a condition of abnormal increased irritability. It is easy to meet the possible objection that the

phenomena observed in these nerves may have been induced by the tetanic disease of the cord—if we may be allowed to use this common expression without prejudice—and not by a pathological condition in the nerve-stems. If that were the case, then they would be demonstrable equally well in the right arm.

If then a pathological condition of this kind exists in the median and radial nerves, we must naturally look for its origin and cause in the wound of the left thumb; which, according to its situation and direction, must have injured fibres of both these nerves. Against the objection that the tetanus as such may again have been the occasion of the increased irritability, the same argument as that already stated is available.

But now, what is the nature of this change in the nerves? In order to answer this question, we may avail ourselves of the frequent observation, made even in the case of wounds for venesection, that from the injury even of small nerve-stems, as by cutting or dividing them, there may develope an inflammatory state in the nerves, a *neuritis*. The symptoms observed would agree perfectly well with the supposition of such an inflammatory process; whether they are sufficient *proof* of its existence is another question. We shall see afterwards how the results of the autopsy bear on this view.

You will all the more be inclined to take for granted the existence of a neuritis in this case, when you consider that, with many negative cases, there are recorded several positive ones bearing in this direction. Thus Lepelletier, Curling, Froriep, Rokitansky have convinced themselves, by anatomical investigation, of the existence of an inflammatory process in the nerves in many cases of tetanus.

On the other hand, there are several more recent observations which make it probable that in tetanus there are distinct histological changes in the spinal cord, but as this is not related to my present subject I may spare you a detailed account of these. If we add to this that Lepelletier fifty years ago proved that in a traumatic tetanus there was medullary and meningeal disease, we shall be led to the view, that the mode of development of tetanus may *possibly* be this: traumatic injury of the nerve-twigs—inflammatory processes in these, extending in the central direction to their respective nerve-stems (neuritis ascendens), and finally passing to the spinal cord.

This would indeed ascribe to the process of neuritis an unusually far-reaching importance, and would make it an affection whose significance extends under certain circumstances much beyond that of a merely local disease. But not only in the case of tetanus, but also in a whole series of other affections, the process of neuritis has lately received an unexpected significance. Take especially, for example, the case of reflex paralysis. This aspect of the subject of neuritis has been lately discussed by Leyden. Inflammation of nerves deserves therefore our careful attention, and I will try, Gentlemen, in to-day's lecture to fill in the symptoms of this disease so as to give you a clinical picture of it.

If our knowledge of the symptoms of a disease were proportionate to the length of time that the disease has been known in the past, then the diagnosis of neuritis would be far advanced towards perfection. For Boerhaave already speaks of it; he says that in the nerves all kinds of phlegmona, erysipelas, etc., may occur; this is, however, on his part only an *a priori* supposition. Since that time the possibility of a neuritis has never been denied, and it has played an important part in the history of the "neuralgias"; but even in Swan's monograph on the local diseases of the nerves, the chapter on inflammation is very inadequate, and the symptomatology stands much in need of the touchstone of pathological anatomy. As I do not intend to give you a detailed history of neuritis I will only briefly select some of the chief data. The observations of Lepelletier and Graves, the fundamental importance of which is only now being recognised according to their merits, first showed the possibility of an extension of the inflammatory process in nerves to the spinal cord, and so we attained to the knowledge of a *neuritis migrans*, in specie *ascendens*, which stands in contradistinction to the neuritis *descendens*. In the latter the inflammatory process wanders from the central apparatus to the peripheral nerves, and may be traced in these downwards into the muscles, as has been lately shown us by some experiments of Klemm. Froriep and Rokitansky then called attention to the remarkable peculiarity, that the inflammatory process does not always propagate itself *per continuitatem*, but sometimes in jumps, localising itself at certain points in the

course of the nerve-stem; we thus attained to a knowledge of *neuritis disseminata*, as it has been fitly named by Klemm (Leyden).

The clinical significance of neuritis was here in Germany brought prominently forward especially by R. Remak; but this distinguished observer based his views almost exclusively on clinical observation; the control of his conclusions by dissection was wanting. It was Kussmaul, and especially Leyden, who, to begin with, pointed out the relation of neuritis to so-called reflex paralysis. Since then neuritis has gained for itself an ever-widening domain, indeed it seems to be often seen nowadays especially from the electro-therapeutic aspect, when its presence is as far as possible from anatomical demonstration.

I will add that we have been furnished during the last few years with several experimental works on this subject, of which two have arisen under Leyden's auspices, namely, that of Tiesler and the excellent inaugural dissertation of R. Klemm; a third is that of Feinberg. I have myself instituted some experiments on neuritis in the year 1868, which have not yet been published. I will touch on the results of these investigations later on, when I shall also have to give you the names of several observers who have concerned themselves with the study of neuritis.

You will perhaps expect from this short historical sketch that we are able to draw a perfectly defined clinical picture of neuritis, but unfortunately this is not the case. The reason of this is that in simple, peripheral neuritis death very seldom occurs, and then only from accidental complications, and so we have only in rare cases the opportunity of controlling the diagnosis by an actual anatomical test; and in the case of neuritis leading to serious affections of the cord, these secondary affections obscure the picture of simple neuritis.

It will be most convenient, in order to a clear view of the subject, to study first the symptoms of this affection, in so far as it remains a local disease confined to the nerves; we will afterwards take up its consequences, those other pathological conditions which are due to neuritis as a cause.

## I.

In the first place then: How does neuritis present itself clinically? In what cases may we and can we diagnose it with certainty?

An inflammatory process may, of course, localise itself in a purely motor, a purely sensory, or in a mixed nerve; in addition, the vaso-motor (trophic) fibres may take part. Perhaps you consider at first sight, that such a careful division is unnecessary, since the affection, let us say, of motor fibres, must produce the same phenomena whether we have to do with a purely motor or a mixed nerve. But in my view this is not correct, because the muscular contractions, and the motor phenomena in general, may, in the case of neuritis of a mixed nerve, arise so far as one knows beforehand, either from the direct participation of the motor fibres or by way of reflex irritation. It is absolutely necessary, therefore, for the due observation and analysis of the symptoms, that this distinction should be made. We must study separately the phenomena which occur in inflammation of a purely sensory and in that of a purely motor nerve, in order to be able to understand the picture presented in the common and most frequent neuritis, that of a mixed stem.

There is another point which requires to be still more sharply accentuated. To state it briefly, it appears to me, Gentlemen, that the diagnosis of neuritis is too commonly based on the etiology of the concrete case, and on a set of symptoms constructed *a priori*. Indeed in studying the literature of the subject one gets the impression as if authors occasionally made this diagnosis according to their own fancy; as by further perusal of the cited cases, this diagnosis is sometimes quite inconceivable, or at least the certainty with which it is brought forward is astonishing. When we observe how in many cases which were, during life, considered to be cases of neuritis, there is, after death, no trace of inflammatory processes to be found in the nerve and its coverings, we are compelled most rigidly to determine that we shall construct the picture of neuritis only after cases in which its presence has been proved by an autopsy, or at least an unequivocal symptom was present during life, namely, the

*swelling of the nerves.* This may seem self-evident, but it is remarkable how often it is forgotten in this particular department of pathology. How often, for example, is it said in the case of certain neuralgic phenomena, that these are based on an inflammatory change, they could only be explained on such a supposition—and a confirmation by the autopsy is then no longer desired! The inference *may* often be correct; but the only scientific method is exactly the opposite, namely, that we should construct the diagnosis *ex posteriori* from the neuritis proved at the autopsy. It is true that we have not very many autopsies of cases of pure neuritis; yet the views which we attain by this method, though they may be limited in extent, have the great advantage of certainty and trustworthiness.

Taking up this standpoint, let us now endeavour to establish *the phenomena which occur in* INFLAMMATION OF A SENSORY NERVE. This expression "sensory" refers of course only to the absence of motor fibres; because every sensory nerve contains vaso-motor (trophic) fibres, and these will come up for consideration in inflammations even of "purely sensory" nerves.

We have very recently come into the possession of a very careful anatomical investigation of a neuritis of the trigeminus by Wyss; there were well-marked inflammatory changes in the Gasserian ganglion, and the first branch of the trigeminus. Unfortunately the clinical symptoms in this important case, which constitutes a pure primary neuritis, are very briefly given. The patient complained of headache, which was very violent and consisted of sharp "burning"; there was sleeplessness, loss of appetite, coated tongue, swelling of the abdomen, constipation, evening temperature 100·2°. After three days a herpes zoster began to develope; and to this succeeded swelling of the upper eyelid and the conjunctiva, with desquamation and localised opacity of the cornea. In another case contributed by Weidner, a zoster also developed in the region of the first branch of the trigeminus, with simultaneous signs of inflammation in the eye and acute fever; the patient also suffered from paroxysmal spasms of pain in the region of the same nerve. The zoster died away; but the patient was subject to a "violent neuralgia" up till his death, five years later. On post-mortem examination there were found undoubted traces of inflammation (which we shall not describe here) in the first

branch of the trigeminus and, perhaps, also in the Gasserian ganglion. In this case also the clinical symptoms are rather briefly related.

It would take me too far, Gentlemen, were I to describe all the cases of zoster with post-mortem, including those of zoster intercostalis. Let us only keep in mind the general results; in several cases in which there were, during life, pains, often of a well-marked neuralgic character, with herpes zoster, sometimes accompanied at the outset with distinct fever, there has been found post-mortem an acute or chronic neuritis (often with gangliitis), and this determined anatomically.

We know, however, that sensory affections (pain and paræsthesia) are by no means constant in zoster, but that about the half, perhaps even two-thirds, of the cases run their course without these symptoms. On the other hand we know further, that in the trigeminus and in mixed nerves an actual neuritis with violent pain occurs without zoster. We must not, therefore, unduly exaggerate the significance of the latter in the diagnosis of neuritis. We may, I think, express the truth in the following formula, basing on the above facts of experience:

*When along with an undoubted peripheral affection in the region supplied by a sensory or mixed nerve, there is a simultaneous occurrence of zoster and sensory disturbance, then we can infer the existence of an inflammatory process in this nerve.*—So far as our present subject is concerned I may leave altogether out of consideration the question whether the nerve alone is affected, or whether for zoster, gangliitis is of consequence, etc.

In connection with this let me observe that the slight appearances of inflammation in the eye observed in cases of neuritis of the first branch of the trigeminus are to be set down as analogous to the zoster. Both of them occur in acute neuritis, in chronic there has been no case as yet observed in which at least zoster was present. On the other hand in chronic neuritis there have been observed a whole series of trophic or vaso-motor disturbances, which are quite analogous to those so often noted recently in connection with injuries to the nerves. It is at the same time to be remembered that in simple chronic neuritis without solution of continuity of the nerve, we do not observe the more serious of these trophic lesions (ulceration of the skin, etc.), but only the less severe,

that is to say, the epidermis becomes cracked and dry, the nails do not grow properly, become yellow and cracked, the hair grows at first and afterwards falls out. Of course these lesions may also be absent. Their diagnostic significance may be thus expressed:

*In cases of undoubted peripheral affection of a sensory or mixed nerve, if we wish to distinguish whether it is so-called simple neuralgia without definite change, or neuritis (chronic) then the presence of trophic disturbance in the hair, nails, or skin, is in favour of the latter.*

It appears to me from some cases possible and probable that a similar diagnostic value is to be attached to simple vaso-motor phenomena in the form of paleness and coldness of the skin, but it has not yet been determined post-mortem. On the other hand we see in undoubted cases of chronic neuritis the opposite condition, œdematous swelling of the skin and livid redness.

It is to be expected that in neuritis of a sensory nerve we should have, above all, *disturbances of sensation.* Unfortunately, however, even in the cases of neuritis of the fifth which have undergone the most perfect anatomical investigation, the statements concerning the affection of sensation during life are for the most part rather brief. Also in those few cases where, after excision of a portion of the trigeminus for neuralgia, a neuritis has been proved histologically in the excised piece, which cases would be very suitable for our purpose, stress has been laid, as was natural, less on the description of the clinical symptoms than on the operative aspect of the case. It is fortunate that we have certain other means by which this gap in our knowledge may be filled up. For in the first place we can, according to the formula given above, make use of well-observed cases of zoster in which there was affection of sensation, in our study of the symptomatology of neuritis, even though there was no autopsy. And further we may without doubt take into consideration cases of neuritis of mixed nerves, when we are studying the lesions of sensation alone.

Patients have always in inflammation of a sensory or mixed nerve *spontaneous pains.* The intensity of these seems to vary; they are generally described as acute, especially in acute

neuritis. In acute neuritis we may have the most severe degree of pain, described as intolerable.

In the mode of occurrence and character of this pain, there are some peculiarities worth noting. They have it in common with neuralgic pains from the most different causes, that the pain affects the region supplied by the nerve involved, and that when very intense it may radiate to the regions of other nerves. But the following is of importance.

In all well-observed cases in which the neuritis was either proved post-mortem, or could be taken as certainly present from the undoubted swelling of the nerve, the pain is described as *almost continuous;* in acute neuritis the patient may be in pain day and night. There do indeed occur, on the one hand, remissions to a certain degree, and on the other hand, occasional violent exacerbations; but perfectly free intervals, the reduction of the pain to a minimum, as we see in "pure neuralgias," do not occur. In cases which have come under my own observation, and which I was able to diagnose as acute neuritis, the patients complained of uninterrupted violent pain, which was temporarily increased by external agents—movements, coughing, etc. In chronic neuritis the condition is similar, only here the pain appears to be on the whole milder.

Do not mistake me, Gentlemen. I do not say that such continuous pains certainly indicate in the concrete case a neuritis, I do not deny that in other affections of a sensory or mixed nerve they may occur. I will rather express it thus:

*Pains which absolutely intermit are evidence against neuritis; continuous pains (even with paroxysmal exacerbations) are to a certain extent and in connection with other phenomena in favour of it.*

A few years ago I had under my care a girl twenty-two years of age, very strong and otherwise healthy, who suffered from a quite characteristic sciatica of the left side, which had come on without apparent cause. The pain occurred in distinct paroxysms with intervals of perfect intermission, and it radiated from the neighbourhood of the left trochanter to the foot, following the course of the sciatic. But *always*, even in the freest intervals, the patient continually felt a *spontaneous* pain at a circumscribed spot behind the left trochanter. This spot was

also, even in the intervals, very painful to pressure, and it was here that the first pain, at the beginning of the disease, appeared. No periostitis or other local disease which might have produced pressure on the sciatic, could be discovered on the most careful examination. The most various modes of treatment were tried for eight months without effect; but four leeches, applied to the spot referred to, mitigated the pain, and then the patient herself applied twenty leeches to the painful spot, remained some days in bed, and was perfectly well.

We have no right to draw a forced conclusion from this case without the autopsy. But it seems to me, everything considered the most likely supposition, that we had here a localised neuritis (or rather perineuritis). If this is correct, then we would require to amplify the above proposition and say: in circumscribed neuritis there may occur neuralgic paroxysms extended over the entire nerve with free intervals, but besides this there exists *continuous pain at the inflamed spot.* At any rate this point deserves further careful investigation.

I must not conceal from you, however, the fact that our proposition to the effect that in inflammation of sensory and mixed nerves there always occurs pain, *appears* to be liable to exceptions. Bernhardt relates the case of a man who, after typhus exanthematicus which had been accompanied with high fever and *severe head symptoms,* suffered suddenly from paralysis of the radial. The patient died some weeks after from a complicated lung affection. There was found at the place where the radial turns backwards in the upper arm an exquisite circumscribed perineuritis to the extent of 2½ centimetres (an inch) with an almost complete degeneration of the nerve-fibres in this spot; further down the connective-tissue sheath of the nerve was normal, but the fibres were in the well-known state of degeneration which we see after severe injuries to nerves; above the inflamed spot the radial was normal.

In this case there was no pain, or at least none is mentioned in the clinical history. Does not this go against our proposition? Let us look more closely at the case.

In the course of the typhus the sensorium was seriously involved, and after the fever had gone the paralysis suddenly occurred. At the time of the occurrence of the paralysis the degeneration of the nerve must have already reached a high

degree, at least at the inflamed spot; the inflammatory process must therefore have begun earlier, that is to say at a time when both fever and dulling of the sensorium existed. In other words, there may very well have been pain present at the beginning of the inflammation, which, however, in the insensible state of the patient was not felt or not expressed. And we can also understand well enough the absence of pain at a later period. For at the inflamed spot, where the nerve was swollen and thickened, where "between the fibrillæ of connective tissue, numerous vessels distended with blood-corpuscles, as well as masses of free blood-corpuscles," were to be seen, at this spot the perineuritic process had no physiological point of attack at which to produce pain by pressure, because the nerve-fibres were here degenerated, are to be regarded physiologically as equal to dead fibres.

I cannot therefore discover in this case any contradiction to our proposition as to the diagnostic importance of pain. We may well deduce from it the further conclusion; that *in long-protracted neuritis a period may arrive when the spontaneous pain, and the pain on pressure* (about to be discussed) *may disappear*, that is to say when, in consequence of the perineuritic or neuritic processes a *complete destruction of the nerve-fibres has taken place.*

The next symptom is of great importance. *Inflamed sensory or mixed nerves are always painful on pressure.* All the cases in which the neuritis was proved at an autopsy or could be considered certain from the swelling of the nerve, teach us this. On the other hand it must remain undetermined whether the whole course of the nerve *in continuo* is always painful on pressure, or whether in neuritis disseminata, only isolated points, the inflamed spots, show this condition.

The diagnostic value of pain on pressure is similar to that of spontaneous pain, that is to say, its absence (subject to the above exception) is evidence against a neuritis, but its presence does not directly prove it, because it occurs in other affections of the nerve-stems apart from inflammatory processes.

The pain caused by pressure shows itself chiefly at the spot concerned; at the same time it mostly radiates out from it, apparently in both the centripetal and centrifugal directions.

Baerwinkel has recently made an interesting remark to the

effect that a centripetal direction of the spontaneous pain as well as of that produced on pressure, arises from the irritation of the nervi nervorum, and indicates an inflammation of the neurilemma. We ought, according to this, to look on the centripetal direction of the pain as a symptom of neuritis.

On this we have to remark that the absence of the centripetal pain is not evidence against neuritis or perineuritis, for in undoubted cases we find only an excentric direction of the pain. Then several of Baerwinkel's cases are too complicated to induce us to attach to them the force of demonstration. But even if we accept some of his cases even without autopsies as really neuritis—and I can offer at least no fatal objection to this —yet his conclusion seems to me very controvertible, and for this reason, that we know wonderfully little about the nervi nervorum, and therefore should hardly construct a hypothesis concerning their presence and function. I believe that Sappey has proved their existence in the sheath of the optic nerve, but it is well known that this has different relations from the sheath of the spinal nerves. Sappey himself has lately deprecated a more general function for these nerves, suggesting that the nervi nervorum are vaso-motor nerves. W. Krause again speaks of nervi nervorum, but also as vaso-motor. Benedict has been, from pathological observation, "forced to conclude, that there are nervi nervorum," and that these are sensory. This is all that I find recorded, and which has been communicated to me from competent sources, on the nerves of the nerves. You will agree with me, Gentlemen, that we have not here enough to build statements on, which are to support others which are still more questionable. Be that as it may, however, I recommend Baerwinkel's account to your further consideration.

To proceed, I attach considerable importance to the condition of the *cutaneous sensibility* in the diagnosis of neuritis of a sensory or mixed stem. As you know, we may often observe in neuralgias, besides the spontaneous pain, changes in the cutaneous sensibility, sometimes hyperæsthesia, sometimes anæsthesia. In fact, in my experience, as I have already stated in a previous publication, this is quite constant, and it occurs also in many different kinds of (intense and continuous) pains from other causes. We may indeed almost lay down a general law to the

effect that at the beginning of neuralgias cutaneous hyperæsthesia is present, and that this afterwards gives place to a diminution of the sensibility. It had previously struck me that this diminution of the cutaneous sensibility sometimes occurred very early in what appeared to be neuralgias. I am now inclined to believe that in those cases neuritis lies at the basis of the neuralgic affection, and the other phenomena found by me in these cases also favour this view. That is to say, the analysis of observations of undoubted neuritis of the sensory and mixed nerves, teaches us that often a reduction of the cutaneous sensibility is very early established, while violent spontaneous pain and tenderness on pressure persist; even in the case of zoster we may frequently demonstrate this while the vesicular eruption still persists.

I cannot tell you on what day the anæsthesia begins to establish itself. That you may make no mistake I repeat that I lay the emphasis on the *early* appearance of the anæsthesia; it may only appear after weeks and still be due to neuritis,—the direct proof and illustration of which you will find in lepra anæsthetici, where we have a chronic leprous perineuritis (Virchow), but at this late date it is no proof of neuritis, as it may occur in neuralgias which have no neuritis at their foundation. We shall therefore formulate the diagnostic significance of this symptom as follows:—

*An early anæsthesia appearing in the course of a few days is in the concrete case, and in connection with other symptoms, evidence in favour of neuritis.*

In how far a *hyperæsthesia of the skin* preceding the anæsthesia is of diagnostic value, we can speak less confidently from experience; at least it seems to me, that since it may occur at the beginning of any form of neuralgia, it is a less trustworthy symptom than the early anæsthesia, even though we may not deny it in a given case all diagnostic virtue.

Let me add, that of course the anæsthesia occurs in the *area of distribution* of the affected nerve, and this as you know does not always correspond with the deep nerve-stem, so that normally sensitive skin may lie over the inflamed nerve, as for example over the sciatic in the neighbourhood of the trochanter. We must indeed go still further, and we say: the anæsthesia is of importance under the conditions formulated

above, IF it is present, but it *need* not be there, or it may at least be very insignificant even in severe neuritis (perineuritis) when it attacks *nerves of the extremities*. This is explained by the vicarious function shown by Arloing and Tripier, which the anastomoses of the nerves of the extremities possess. As these things have lately become generally known, I need not dwell longer on them.

We may conclude here the characteristic symptoms occurring in inflammation of sensory nerves *as such;* some others which it has in common with the neuritis of motor stems, we shall consider further on. We do not need to refer to any of the other subjective phenomena, the feeling as if the area of the affected nerve were asleep, feeling of cold, etc., as these have no characteristic significance.

We turn now to NEURITIS OF MOTOR STEMS. The anatomical results are still more scanty in relation to the inflammations of these than even for the sensory nerves. We naturally turn in the first place to the purely motor cerebral nerves; but we know absolutely nothing of a spontaneous neuritis of the motor nerves of the eye and the hypoglossus; in the case of the accessory of Willis, its existence has been supposed, but not proved. The facial, therefore, alone remains. You are aware, Gentlemen, how prevalent the view is nowadays, that in many cases of so-called rheumatic paralysis of the facial there is an inflammation of the nerve-sheath; it is supposed that in consequence of the inflammatory hyperæmia and exudation, the nerve in its narrow bony canal undergoes pressure, and thus many cases of rheumatic paralysis of the face are clinically to be ranged along with the severer forms of traumatic paralysis. This view seems very probable; and if it were correct, we should have a beautiful type of neuritis of a motor stem. It is all the more unfortunate that—so far as I have found—there is no recorded case of simple rheumatic facial paralysis in which a neuritis was established *post-mortem*. And as we are here desirous of deducing the symptoms of inflammation of motor nerves from cases which have been established either anatomically or at least by undoubted clinical facts, we must unfortunately give up rheumatic facial paralysis.

It is plain that we cannot avail ourselves in our study of

this subject of the inflammation produced by wounds of the facial. We may more fitly use cases in which a neuritis of the stem of the facial has been produced by caries of the petrous bone, where this has been anatomically determined, as in a well-observed case by Erb. Again, experimental observations cannot decide the questions which arise in the study of the inflammation of purely motor nerves, as in rabbits and dogs, the ordinary experimental animals, the facial and accessory contain sensory fibres, as the recent experiments of Arloing and Tripier have shown. We must therefore study the symptomatology of neuritis motoria—if we may use such an expression—in mixed nerves.

If we required to exercise care in availing ourselves of the functional disturbances of the sensory nerves for purposes of diagnosis, it is still more necessary in the case of the motor stems to make a careful discrimination in the concrete case. This applies in the first instance to the so-called motor phenomena of irritation, the *twitchings* and *contractions*. It is always to be remembered, that these may be altogether absent in undoubted neuritis. In other cases they are actually observed; but we are not in a position to determine with certainty, under what circumstances they occur, and what special significance is to be attached to their occurrence. We can just as little say with certainty how they arise; whether, that is, they originate by reflex action or by direct irritation of the motor fibres; and if the latter be the case, whether they are caused by an interstitial inflammation (perineuritis) or by an affection of the nerve-tubes themselves. However probable may appear *a priori* the supposition of a direct motor irritation, it is still opposed by the fact that contractions are mostly absent at the beginning of so-called rheumatic facial paralysis—supposing that we have here really a perineuritic process, which, as we have said, is by no means completely established. We may attach indeed more weight to the few cases of neuritis facialis from other causes, as from caries of the petrous bone, which have been confirmed post-mortem, and in these there is also no mention of primary contraction, but paralysis was immediate. This would likewise bear against direct irritation of the motor fibres as a cause of contractions. We must leave this question an open one.

If we add to this that contractions may occur in non-neuritic processes, then you will easily see, Gentlemen, the small diagnostic value of this symptom, which is only of consequence in connection with other phenomena. As a clinical fact it is observed that there occur in the region of the inflamed nerve-stem, either occasional spasmodic twitchings of the muscles innervated by it, or a more tonic contraction of these.

Of much greater importance than the phenomena of irritation are the *paralytic and paretic states*, for the diagnosis of neuritis of mixed nerves. We do not consider here, of course, cases in which a direct traumatic action occasioned the inflammation (cuts, pressure, etc.), because in such cases the injury, as such, may have produced a local interruption, or at least impediment to the conduction.

This disturbance of movement may present different degrees, and from a slight feeling of fatigue only appearing when movements are made, it may increase to complete paralysis. Two possibilities exist, and are in reality active in its production. The interference with motion may on the one hand be the effect of compression of the nerve-tubes, where the process is chiefly perineuritic, or on the other hand it may be the result of a direct inflammatory change in the nerve-fibres. Observation teaches that, as a general rule, the more severe cases of paralysis belong to the latter category, the slighter paretic disturbances to the former. We may further say that *in general* the states of motor paralysis arise after the case has had a somewhat longer duration, the states of irritation belong more to the outset.

It would be a *mistake*, however, to imagine that in neuritis (or in this case more properly perineuritis) paralysis *must always occur*. If the nerve-tubes are not at all involved directly, and if the inflammation in the connective tissue-sheath only attains a slight degree, then every notable disturbance of motion may be absent. But this is sometimes the case even in more severe perineuritis. Virchow, for instance, says, "I have in many cases of tubercular leprosy found the ulnar at the elbow swollen and painful without anæsthesia or paralysis being present." And we know from Virchow, Boeck and Danielssen that these swellings of the nerves in leprosy depend on a chronic perineuritis. On the other hand, analysis of all the cases where microscopic examination has revealed a considerable

affection of the nerve-tubes themselves, has shown that under these circumstances there always occurs a more or less pronounced disturbance of motor function.

We may summarise what we have just said as follows: *Motor paresis or paralysis may be present in inflammation of motor and mixed nerves, but it is not an absolutely necessary symptom.* If it be entirely absent, and if the inflammation can be otherwise certainly diagnosed, then we may conclude that the nerve-tubes themselves do not directly participate; on the other hand the existence of interference with motion does not prove that the connective tissue-sheath is not involved, because it may occur in severe perineuritis (by means of the disturbances produced by compression).

I have another symptom of neuritis of motor nerves to adduce, which has lately been specially attended to, and considered in its relations to neuritis as a whole—I refer to *atrophy of the muscles supplied by the affected nerve.* In order to illustrate this point, allow me to bring before you, at least in its principal features, a case of this kind observed by myself.

A cook, twenty-five years of age, unmarried, presented herself at the beginning of June, 1873. She had been previously healthy. Thirteen weeks before, being pregnant, she entered the lying-in clinique on account of swelling of both legs; eight weeks after, there occurred a normal and easy delivery, after which the swelling of the legs disappeared. About twenty-four hours after delivery, patient detected, as she was in the act of placing her child to the breast, a pain in the right upper arm and the anterior half of the right side of the thorax, which very much interfered with the movements of the right arm. Soon after, she felt, in lying, another pain in the region of the right scapula and beneath it, and this also was increased by movements of the arm. Some days after delivery, but subsequent to the pains already mentioned, there supervened a slight inflammation of the right mamma, which however soon disappeared under the simple application of wadding. Fourteen days after the confinement patient resumed her previous occupation, although the pains still continued and the paralysis of the arm rapidly increased. About this time also pain and a slight feeling of tiredness appeared in the left upper arm. The patient could assign no cause for her illness.

The essential results of the examination at the beginning of June, after the affection had lasted five weeks, were briefly as follows:—

On the right side great atrophy of the deltoid, slight atrophy of the muscles of the shoulder and the pectoralis; complete absence of function of the deltoid and serratus anticus major, diminished power in the pectoral, supra-spinatus and infra-spinatus, subscapularis, teres major and minor. The movements of the elbow and wrist, as well as of the fingers, are perfectly normal. Pressure on Mohrenheim's fossa on the inner aspect of the superior third of the upper arm and on the outer aspect of the head of the humerus produces severe pain. The spontaneous pain, which was at first very violent, has become gradually less, and now only occurs when the arm is moved, but then it generally lasts a considerable time. No certain result is obtained by testing the sensibility, but it seems to be slightly diminished on the right. Otherwise there is nothing abnormal to be found. It is doubtful whether a commencing atrophy of the muscles of the thumb and the interossei in the hand exists, at least there is no difference in function as compared with the other side.

The patient did not come back till the beginning of November. At this time there existed great pain over the right clavicle, motion and nutrition of the muscles were not essentially changed. At this time leeches and dry cupping were several times applied to the painful spots and the constant current used; the patient again stayed away.

When the patient again showed herself in February, 1874, after about four months, there presented itself the following surprising condition, without any treatment having been used in the interval: The pain has quite disappeared, and so have the atrophy and immobility. All movements are again possible; their strength is quite normal; the state of nutrition of the formerly atrophic muscles leaves nothing to be desired.

I shall not attempt an analysis of all the diagnostic possibilities in this case. It was most natural at the first investigation to think of a commencing progressive muscular atrophy in the common sense of this expression. But even at that time many circumstances were against this supposition, especially the remarkably violent pains and the rapid course; this dia-

gnosis has, in my opinion, been entirely set aside by the further course, the perfect and almost spontaneous cure. In view of all the circumstances, I think we have had in this case to do with inflammatory processes in the nerves of the atrophic and paralysed muscles. We cannot, indeed, directly disprove the supposition that here the primary disease was rather myositis than neuritis, inflammation of muscle rather than of nerve; but this supposition itself stands in need of proof. The facts seem to be in such a position that reasons could be adduced on both sides, and the determination would lie with the individual observer.

But, Gentlemen, we possess actual evidence for the statement that muscular atrophy may be a result of neuritis, and under certain circumstances it may be a symptom of high diagnostic value. This evidence exists not so much in pathological observations which, in regard to this point, are very meagre and somewhat indefinite, but rather in the experimental results which have been gained by several observers; for example, Hjelt, Neumann, Vulpian, Erb, and with special reference to this question, Klemm.

As I have already indicated above, the last-mentioned author has determined, by experiments on cats and rabbits, that in almost every instance, "whether the agent giving rise to the inflammation was applied to the nerve-stem or the centre, the muscles supplied by the neuritic nerves were affected." In the acute cases the muscles showed even to the naked eye appearances of acute inflammation; in chronic cases where death occurred after the neuritis had existed for several weeks, a distinct atrophy of the muscles resulted. The more intimate pathogenetic relations of this atrophy, the mode of their connection with the neuritic process, we may here leave out of consideration, it is enough for our present purpose that the fact is established, that *primary neuritis may induce secondary muscular atrophy.*

Friedreich and some other authors had already from existing material, partly clinical and partly experimental, come to the same conclusion before Klemm's direct experiments proved it.

You will readily understand what importance these results, established by experiment, must have in relation to cases similar

to that which I have just related. It is possible (for I may not speak more definitely in the absence of autopsies) that these experiments may come into account for many of the well-known cases of muscular atrophy which sometimes develope in the course of "neuralgias," especially in sciatica. Indeed, Friedreich is inclined to refer "all cases of neuralgia in which muscular atrophy supervenes, to neuritic processes." Laségue has already done the same, and quite recently again, Landouzy distinguishes a "neuralgiform," and a "neuritic" sciatica, the latter occurring with atrophy. It is true that the French author does not adduce a single autopsy in support of his view, and not even any experimental results.

Of wider interest is the question which has been much discussed, whether the atrophy which, after injuries to nerves (severe contusion, division,) follows on the paralysis, depends on a descending neuritis, or has to be otherwise explained. However important this question may be from a general point of view, it is in relation to diagnosis of subordinate consequence, for in these cases the etiology of the lesion is plain enough, namely, the direct injury to the nerve and the interruption of the conduction. When after an injury of a nerve-stem paralysis and atrophy occur more gradually, and it cannot be supposed that there was any immediate interruption of conduction, then it would be very important for diagnosis and for our comprehension of the case, to know quite certainly that a neuritic process is always the intermediate and determining link, but this has as yet only a high degree of probability in its favour.

I need hardly add, that this atrophy need not always follow, even in actual neuritis. And to avoid any mistake I will further remark, that according to our present state of knowledge, other causes besides neuritis may produce atrophy, even in cases of purely peripheral disease of the nerves.

We may formulate these results as follows :—*In an undoubtedly peripheral affection of a mixed or motor nerve, muscular atrophy in the region of the affected nerve taken in connection with other symptoms pointing in that direction, is evidence in favour of neuritis; its absence, on the other hand, is no evidence against neuritis.* We do not here take into consideration traumatic cases with interruption of the continuity of the nerve.

These are the most important and essential symptoms which

neuritis produces according as it attacks a motor or sensory nerve. It is plain that they will be combined when a MIXED NERVE is involved. It is, however, remarkable that usually in inflammation of a mixed stem, as almost all the nerves of the extremities are, the sensory phenomena are the most prominent, at least *at the outset*, so that not at all infrequently the symptoms are those of a "neuralgia." I do not at all intend to enter here on a more particular discussion of the neuralgias. You are aware that at one time all neuralgias were referred to neuritis. It is at once apparent that this view is incorrect, if you compare the group of symptoms of neuralgia with those which we have endeavoured to describe as belonging to actual neuritis, and it is shared in by none of the better pathologists. It will be difficult sometimes in the concrete case to decide whether or not a neuritis lies at the basis of a neuralgic group of symptoms. Yet I do not need, after all that has been said, to bring forward more particularly the points on which the differential diagnosis has to be made. And it is likewise unnecessary that I should give any special description of the symptoms of neuritis of a mixed nerve; this is readily deducible from what has already been said.

Let us only consider further in a few words certain phenomena which may occur in every inflammation of a nerve, whatever the physiological function of the nerve may be. These are PHENOMENA WHICH ARE DIRECTLY RELATED TO THE ANATOMICAL PROCESS AS SUCH.

The most important of these is *swelling of the inflamed nerve*. This does not occur in every case, and is often not to be felt on account of the concealed position of the nerve; but when it can be felt with certainty, and can be referred to the nerve itself, it forms one of the most important and trustworthy symptoms of neuritis, so important that it may, by its presence, establish the diagnosis with certainty.

The swelling is produced in every case by perineuritic processes, at least I know of no case in which, with an affection of the nerve-tubes alone, without the connective tissue-sheath being involved, there has been any thickening worth mention, or such as could be detected during life by palpation.

In some cases we feel the nerve as a continuously thickened cord; I was lately able to demonstrate this condition to you in

a patient, who suffered from a neuritis affecting the crural nerve, and which had begun in an old stump. In other cases the thickening occurs at intervals, at certain spots in the course of the stem (*neuritis nodosa*). In the artificial inflammation in rabbits and cats, Klemm was able to determine certain seats of election of the inflammatory process, and these corresponded to various stages in the advance of the neuritis, which was by fits and starts. These spots were in those animals the places where branches passed from the neighbouring arterial stems to the nerves, and they had their position mostly near the joints. We must unfortunately refuse to go into the subject of the neuroma and its frequent genetic relation to chronic neuritis, because it would take us too far from our proper subject.

Of less importance, because more equivocal and untrustworthy, are some other symptoms.

To this class belongs a slight *erythematous redness*, which sometimes occurs, but only in acute neuritis. This may be ribbon-shaped, corresponding to the course of the affected nerve-stem, and thus resembling a superficial lymphangitis; or it may perhaps be distributed over a larger area, as in the case of our patient above referred to, in whom the greater part of the front of the thigh was slightly reddened.

Again it is only in acute neuritis that we have any *febrile* disturbance, and this is sometimes slight, but sometimes of very considerable intensity. As observations are defective on this point, this simple notice will suffice.

We have finally to refer to the *electric condition*, but here also our results are by no means conclusive or certainly established. In general it is supposed that at the outset and in slight cases there is an increased irritability for Faradic and galvanic currents (Erb); in chronic cases there seem various conditions possible. In the slighter forms of perineuritis there seems to be no change from the normal. The severer cases, that is to say, those with direct lesion of the nerve-fibres, and degeneration of these, present similar conditions to those which we are all acquainted with in traumatic paralysis of peripheral nerves. At any rate we shall ascribe to the electric condition no distinct diagnostic value, its importance is limited and only of use in connection with other symptoms.

Now that we have discussed how a neuritis presents itself

clinically when we can diagnose it, let us return, Gentlemen, with the views we have thus attained, to our case of tetanus. We thought that we could infer from the phenomena the existence of a neuritis ascendens of the median and radial nerves. But if you now compare these phenomena once more with those which we have learnt to consider characteristic, and which alone entitle us to diagnose a neuritis, you will be compelled to say: the symptoms in our case are indeed consistent with the supposition of a neuritis, do not directly contradict such a diagnosis, but they do not at all *prove* its existence.

If this case had not come to a post-mortem, we should perhaps have looked on it as a fresh confirmation of the view, that traumatic tetanus may be set up and caused by a neuritis ascendens proceeding from the wound. But the *autopsy* has given the following results: the median and radial nerves show to the naked eye nothing abnormal. Microscopic examination prosecuted by my respected colleague, Prof. W. Müller, and myself did not show either in fresh or hardened preparations, anything that could be referred to neuritis or perineuritis; no trace of inflammatory processes in the sheath or neurilemma, and nothing abnormal in the nerve-tubes could be recognised. The Pacinian bodies also in the neighbourhood of the wound showed nothing special.

We therefore found none of the anatomical changes which belong to perineuritis and neuritis. We must therefore cast away this supposition, and can only say in general terms, that the phenomena observed in these two nerves during life must be referred to an *increase of the irritability;* this has been doubtless set up by the wound, and may be brought into relation with the development of the tetanus. What this further connection may be, and what ultimate molecular changes exist in the nerves, are matters which are beyond discussion.

We would still however deduce two important conclusions from this case:

1. We must not, when the properly characteristic phenomena are absent, diagnose a neuritis from a simple increased irritability, even when (as in this case) the etiological relations readily suggest such a conclusion.

2. In traumatic tetanus we may have a functional increase of the irritability in nerves encountered by the wound, without

finding the anatomical evidences of neuritic and perineuritic processes.

## II.

The case with which we set out in our description of neuritis, and the possibility of connecting this whole discussion with tetanus, must have taught you, Gentlemen, that inflammation of nerves has not always the significance of a merely local process, which may under various circumstances be slighter or more severe, but always calling forth phenomena confined to the region of the affected nerve (pain, paralysis, atrophy). We have, as I have already indicated in a short historical sketch, come in recent years to recognise that, as a result of neuritis, and through it as a connecting link, there may occur a series of further processes. Some part of this is sufficiently established, other parts are chiefly in the state of hypothesis as yet, and await a definite settlement.

In the first place we have the remarkable fact which for some regions of nervous pathology is of extraordinary importance, that on an affection of a peripheral nerve which was originally local, certain further neuropathic states may supervene, and this from an anatomical peculiarity of neuritis, which it shares with many affections of the spinal cord. This peculiarity consists in the tendency of neuritis to wander, to extend from its original seat, in the central and peripheral directions. There was hardly a doubt as to this fact from the results of pathological observations, but it has been incontrovertibly established by the experiments of Tiesler, Feinberg, Klemm, of which those by the last mentioned author are most striking and multifarious. These experiments give, in brief, the following results:—

Neuritis may extend downwards, and pass to the muscular substance, which finally, in chronic cases atrophies—we have already considered this aspect of the experiments. But the inflammation may extend upwards and reach the spinal canal; in this case the connective tissue surrounding the dura mater is most frequently involved (peri-pachymeningitis), but the cord itself may be directly concerned, either with or without affection of the pia mater (myelitis and myelo-meningitis). In some experiments of Feinberg and Klemm the inflammation propa-

gated itself even to the encephalon. It is particularly note worthy that a continuous propagation of the inflammation is not necessary for the production of this secondary spinal affection; it is even more frequently allied with neuritis disseminata. Finally, Klemm's experiments have given the very important result that the inflammation may be propagated from the nerve-stem of one side to that of the opposite side, and even to another extremity. The spinal cord and its membranes are frequently involved in these cases, but it may be otherwise. This is the form designated *neuritis sympathica*. We shall see immediately that clinical observation furnishes manifold illustrations of these experiments.

*In all cases of the kind hitherto before us the secondary affection is conditioned by the propagation of the anatomical process.* But we are in the meantime compelled from the present state of our knowledge, and in view of actual clinical experience, to admit another mode of origin of these secondary affections following neuritis. Further on I shall point out to you, that it has been several times observed that the so-called central neuroses, that is, affections whose anatomical basis is still unknown, may arise in connection with and be determined by a peripheral neuritis. At present we cannot ascribe any other meaning to these cases, without being arbitrary, than this, *that the abnormal state of irritation in the inflamed nerve raises up and induces in predisposed persons, those secondary affections without any coarse anatomical lesions.* Before we go further into these considerations, let us now in the first place take up the facts concerned.

Last session I brought before you a married woman, aged thirty-three, who complained of pain in both legs, and great difficulty in walking. The following is the history of the case: Eight weeks before, she had, on turning quickly, "sprained" the left foot; she felt a violent pain in the left ankle-joint, which after some days of rest disappeared completely. Two weeks later, she experienced while walking a slight dragging pain in the back of the left foot, which extended thence to the outer side of the leg as high as the knee, but did not interfere with walking, and gradually subsided. Since then pain frequently occurred, especially at night, on both sides of the dorsal vertebræ, which was more severe when she lay on her back. Two weeks later when again taking a short walk, the pains in

the left foot and leg appeared to be more severe, and interfered with her walking. From that time onwards for a space now of four weeks this condition remained, and compelled patient to lay herself up. On asking her to walk it appeared as if the left foot bent inwards, and she fell in that direction. By degrees she noticed that she was able to use the left foot less and less; it assumed a slight equino-varus position. After a time a darting pain passed through the outer and posterior aspect of the left lower leg, even when lying at rest. For some three weeks she has experienced pain in the neighbourhood of the left tuber ischii. Also a padded feeling appeared in the left foot and lower leg; the pains in the back mentioned above have also returned.

Within the last fourteen days there have appeared quite similar *pains in the right calf.*

The following was the state on examination:

The left leg is emaciated: at the calf the difference is one centimetre (2-5ths of an inch); subcutaneous fat on both sides equally thick; temperature and colour of the skin the same on both sides. The whole course of the sciatic from the ankle upwards is painful on pressure, less so in the thigh than lower down; on the other hand pressure in the neighbourhood of the sciatic notch is again very painful. On the right side also the point of exit of the sciatic is painful on pressure, and so is the right calf. No distinct swelling of the nerve is to be felt. Nothing abnormal is visible in the vertebræ; but pressure causes pain from the eighth dorsal vertebra downwards, but not above this; further, pressure on both sides in the neighbourhood of the lumbar vertebræ is painful.

The patient is able to move the right leg normally. The left leg is only moved with some trouble, and trembles somewhat, active extension of the left ankle is quite impossible, flexion is diminished; in bed the foot lies in the equino-varus position. The sensitiveness to pricking and to variations of temperature is diminished in the left lower leg, especially behind, and also on the posterior aspect of the thigh.

The electric irritability is diminished on the left side both for direct and indirect stimulation, very much so for Faradisation, moderately for galvanism.

The diagnosis of this case should, in view of the results of

experiment, be pretty clear, so far as we can decide without an autopsy. I think we have to do here with a traumatic neuritis occasioned by a subluxation of the ankle, the neuritis has taken an ascending course, has probably involved the membranes of the spinal cord, and secondarily also the nerves of the right side. If the supposition be correct, then this case affords a further confirmation of the views expressed by Remak and Leyden, to the effect that—so far as our present experience reaches—*direct traumatic neuritis and that induced by inflammations of joints, present above all others, the tendency to extend, and to give rise to secondary affections.* In the so-called spontaneous (rheumatic) neuritis this is much more rare; and Leyden asserts, with special reference to leprosy, "that the inflammations of nerves with extensive anatomical lesions, are just those which show a slight tendency to extend further (to the central apparatus)."

Several recorded observations *seem* to show (we cannot speak more definitely without post-mortem) that, just as in the experiments, secondary neuritis (sympathetic) may occur *without the spinal cord being involved.* To this class appear to belong certain cases of double neuritis disseminata of the sciatic without spinal symptoms. We should have then a group of cases in which the inflammatory process showed a propagation to other nerves *per saltum.*

Duménil made an interesting communication ten years ago. He observed the following case which was afterwards controlled by a *post-mortem,* and whose course could be thus construed *a posteriori.* In consequence of an injury there developed a manifest chronic neuritis of the sciatic with the most complete series of symptoms (pain, motor paralysis, muscular atrophy, anæsthesia). Quite gradually there developed in the course of years, neuritis with similar symptoms in various nerves of the other extremities. On post-mortem examination there were found, besides neuritis, very distinct alterations in the cord and its membranes, thickening of the dura and pia mater, atrophy of the nerve-roots, occasional changes of the white substance, but especially of the grey substance, all which Duménil considers to be effects of chronic inflammation. We pass over the clinical and anatomical details; and I also spare you the rehearsal of some other cases of Duménil and other authors, because being

without autopsy they prove less, although in the light of this case of Duménil, which came to a post-mortem, they are quite plain.

From this case it follows without doubt, that the primary lesion here was a neuritis ascendens and descendens of the sciatic nerve (the injury had acted on the nerve in the neighbourhood of the tuber ischii), and that the secondary lesion was the chronic affection of the cord. It is more difficult to determine what may be the connection of the inflammation in the other nerves with the original sciatic neuritis. There are here two possibilities. Either the course has been as follows: primary neuritis of the sciatic with secondary affection of the cord, especially in the region of the roots of the sciatic, then an inflammation spreading from this to other parts of the cord, and causing ultimately a neuritis descendens in the nerves of the other extremities. Or we have had this course: primary neuritis of the sciatic, with secondary spinal disease remaining confined to the region of origin of the sciatic; then sympathetic neuritis of other nerves, which again produced spinal disease in their region of origin. Whichever way this question may be decided, we have at least in cases of this kind, *chronic (inflammatory) affections of the spinal cord consequent on primary neuritis.*

In connection with this it may be remarked, that according to a view recently expressed by Friedreich, neuritic processes play an important part in the classical disease to which Aran and Cruveilhier first gave the name *progressive muscular atrophy.* You are aware that Friedreich holds the order of the anatomical processes in this disease to be; first a primary chronic myositis or inflammation of the muscle; this calls forth a neuritis predominating in the intra-muscular terminations of the nerves, and extending along the nerve-stems in the central direction to the spinal cord, where it finally sets up a chronic myelitic process. This is, of course, not the occasion to discuss this wide question. It is only necessary to inform you what importance has been attached to neuritic processes in the pathogeny of progressive muscular atrophy.

A further group is formed by the cases in which there ensue, not on traumatic neuritis, but on affections of the urinary apparatus, the intestine, or the female organs of

generation, certain spinal (and cerebral) symptoms, mostly paralytic, and in which these secondary spinal (and cerebral) symptoms, are conditioned entirely by the disease of the cord (or brain) without any secondary inflammation in the nerves of the paralysed (or otherwise affected) extremity. As an anatomical foundation for this view there have been demonstrated inflammatory processes of the cord and its membranes, which were called forth by a neuritis ascendens proceeding from the original seat of the disease. These cases include the most of those which are usually comprised under the much-discussed name of *reflex paralysis*. (As reflex paralysis has been already fully discussed by Leyden in the second lecture of this collection, I only make this brief reference to it here and refer the reader to this excellent lecture.)

But we have not yet exhausted the series of secondary affections called forth by neuritis. One other we have already recognised, namely, tetanus. We have previously said that a neuritis ascendens is by no means to be found in all cases of traumatic tetanus; our own case is one in which there was no neuritis recognisable anatomically. But on the other hand, Lepelletier, Curling, Froriep, and Rokitansky have seen neuritis proceeding from the wound and ascending by leaps. There can therefore be no doubt of the fact. Michaud has recently found in a case of tetanus fine nerve-fibres in the sciatic nerve which lay in the immediate neighbourhood of a gun-shot wound, and was at this point distinctly red, swelled and of firmer consistence, but otherwise quite normal. But the question whether these fine nerve-fibres are to be referred to a pathological and eventually to a neuritic process, or whether they are a normal occurrence, as some other observations render probable, but which is not yet quite certain, does not alter the fact which has been sufficiently established, that sometimes in traumatic tetanus a neuritis is met with. In what way the tetanus may be induced by the neuritis, is a question beyond our power of judging.

We come now to a point in our disquisition where the greatest care is required, because the sure foundation of anatomical observation is wanting, and hypothesis must in part take the place of actual observation. I think it nevertheless necessary to take up these questions, because, for one thing, there is a certain amount of actual material in *favour* of these views,

and further because, when attention has been once directed to the subject, at any moment perfect light may be thrown, by means of anatomical results, on those points where now we can only fill the gaps by a combined consideration of facts. You are aware, Gentlemen, that a definite group of cases of epilepsy is distinguished under the name of reflex epilepsy or (as I have preferred in another place, for reasons not to be discussed here, to call it) *secondary epilepsy*. In the majority of the cases of this kind, we have to do with epilepsy which has developed in consequence of injuries of peripheral nerves, either large branches and stems, or merely cutaneous branches. If we endeavour to explain the mode of origin in these cases, there is no doubt, from manifold observations, that in the majority of cases purely functional centripetal processes are concerned, that is, processes without appreciable anatomical changes. In view however of all that we have already learnt as to neuritis and the conditions resulting from it, the question will not only appear allowable, but indeed a necessary one: *whether in the origin of the secondary epilepsies a neuritis may not sometimes come into play?*

The actual material as yet to hand is so defective that it is not possible to give a decided answer to this question. It is only allowable of course to base on cases which have been distinctly made out anatomically, and these are certainly very rare. One of the best is one recently published by Virchow. A soldier was shot in the upper arm, sustaining a lesion, according to the symptoms which followed, of the median nerve. Gradually there developed in this patient, cerebral symptoms, delirium, irregular pulse, and so on, and shortly after, epileptic fits, then a psychical change of such a kind that it became necessary to consider the question of removal to an asylum. At this time a piece of the median an inch long was excised. Virchow found in it an exquisite neuritis interstitialis prolifera. The epiletic attacks and the excitement disappeared after the excision.

It is, according to the course of the case, possible but not probable, that coarser anatomical changes had arisen in the central apparatus. For we do not know whether the neuritis was confined to the neighbourhood of the wound, to the excised piece, or had extended upwards as an ascending neuritis. It is only certain that there was neuritis at the injured part. When

again we take into consideration other cases, as one by Billroth, where likewise an epilepsy developed after an injury to the sciatic, but where the epilepsy disappeared after exposure of the nerve (without excision) and the nerve appeared normal; when we consider such cases it is necessary to be careful in drawing further conclusions from Virchow's case as to the connection of neuritis and epilepsy. A similar case has been recorded by Schnee, but one also which proves nothing absolutely; in consequence of a wound to the head, without the bone being injured, an epilepsy developed, which later on disappeared when, years after, the cicatrix which was painful and on being touched produced epileptic attacks, was excised. In the excised cicatrix there were two nerve-twigs which showed distinct signs of a perineuritis. Quite analogous is an observation by Dieffenbach, in which after an injury to the hand by a piece of broken glass epilepsy occurred; Dieffenbach cut out the contracting cicatrix; at one spot there was still a splinter of glass and the nerve was here thickened and condensed. After the operation the epilepsy disappeared. An observation by Echeverria is perhaps even more instructive than Virchow's. *Without any injury to the skin* there developed in a "nervous" girl, fifteen years of age, after a fall on the right elbow, epileptic attacks and symptoms which pointed to a local affection of the ulnar nerve, perhaps neuritis. Finally after four years a piece of the ulnar, corresponding to the place of injury, two inches long, was excised, and this piece showed both to the naked eye and microscopically evident signs of neuritis. Up till four weeks later, at which time the case was published, there had been no more epileptic attacks.

I will not accumulate cases of this kind, because I cannot bring forward more conclusive ones. Altogether apart from the fragmentary investigations in these cases, we cannot with them, that is, with cases of traumatic origin, determine the principal question: Was the neuritis in them an *essential* link in the chain of phenomena, did it produce as such the development of the epilepsy? Or was it only an accidental complication of an injury which would in a predisposed person have produced epilepsy even without neuritis (from simple sensory irritation of the cicatrix or wound) as we know to be possible from other cases.

The only demonstration that neuritis is necessary, would be a case where a so-called spontaneous not traumatic inflammation of a nerve could be demonstrated as the only source of origin of the epilepsy. We must still in the present condition of our knowledge keep before us the possibility of such a connection.

Still more scanty are the facts concerning other neuroses. In the case of *chorea* we have only one sufficient investigation of the peripheral nervous system (median and sciatic) quite recently by Elischer. He showed certainly the existence of a perineuritis and neuritis of these stems (which were alone examined), but besides, there were diffuse histological changes distributed over the brain and cord. The conclusion is not in any way warranted from the investigation of this case in connection with the clinical history, that the neuritis played a primary part; the histological changes in the whole nervous system seem rather co-ordinate. On the other hand there are cases of chorea which show that abnormal states of excitement of the peripheral nerves may sometimes induce this disease. Thus I brought before you a short time ago a choreic girl, in whom, as Dr. Rosenbach had determined,* there existed distinctly painful points for the constant current in the course of certain nerves. Blisters and electricity applied at these points caused the disappearance of the chorea with surprising rapidity. But there is no evidence that there was neuritis here. The same applies to an interesting case of Hitzig:—A bayonet-wound through the thigh, continued pain in the cicatrix, epilepsy, excision of the cicatrix, gradual disappearance of the epilepsy, continuance of pain and one-sided spasms, development of choreic movements. You see here possibilities, but not the slightest certainty.

And we must take up this same standpoint—possibility, but no kind of certainty—for several other states. Thus it appears possible that *hysteria* may develope in consequence of a neuritis; I possess one observation in which after a traumatic arthritis phenomena appeared which pointed to a neuritis, and afterwards an hysterico-epileptic state. Hitzig recounts a case of which I only give the heading: Laceration of the ulnar nerve, secondary rheumatoid pains, *paralysis agitans*, epileptoid attacks.

* The case will shortly be published.

Koeppe has recently published several observations in which we are asked to accept injuries to the head as the peripheral cause of "reflected" *psychoses*, as Koeppe expresses it. The first case is indeed striking, as, after the excision of the cicatrix perfect recovery occurred. For the view that neuritis is an intermediate link in cases of this kind there is wanting any basis in fact even more than for epilepsy. For, apart from the argument which was applicable in the case of epilepsy, it is expressly stated in what appears to be the most demonstrative case, "no special changes in the terminations of the nerves, neuroma, etc., were found."

But if our knowledge in this direction, that is, the so-called neuroses, is still very defective, we should not at least forget that there is a reason for this in the extraordinarily small number of investigations instituted in this direction. We shall only attain to a final conclusion when numerous observations shall have furnished us with a wide basis of fact. It may be that these observations will some day teach us to recognise the significance of ascending neuritic processes for the development of many forms of neuroses.

*Literature.**—In the above treatise the literature referred to has of course been used; it was necessary however from the form of the lecture, to refer to the various series of authors only by name. The works concerned are the following:—

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*Boerhaave*, Prælectiones acad. de morbis nervorum. Lipsiæ, 1762, p. 52.

*Curling*, A Treatise on Tetanus. London, 1826.

*Dieffenbach*, Operative Chirurgie.

*Duménil*, Gazette Hebdomad., 1866, Nos. 4—6.

*Echeverria*, On Epilepsy. New York, 1870.

*Elischer*, Virchow's Archiv, Bd. lxvi.

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*Feinberg*, Berliner Klin. Wochenschr., 1871, No. 41.

* This has been kindly furnished by the author for this translation.

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*Klemm*, Über Neuritis migrans. Inaug. Diss. Strassburg, 1874.

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*Rokitansky*, Lehrbuch der pathol. Anatomie.

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*Swan*, A dissertation on the treatment of morbid local affections of the nerves. London, 1820.

*Tiesler*, Über Neuritis. Inaug. Diss. Kœnigsberg, 1869.

*Virchow*, Die krankhaften Geschwuelste, and in his Archiv, Bd. liii., 1871.

*Vulpian*, Arch. de physiol. norm. et pathol., 1869.

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# TRANSFUSION AND AUTOTRANSFUSION.*

BY

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GENTLEMEN,—In the Surgery of the last few years there are three departments in particular, towards the cultivation of which a certain predilection has been shown by authors. If, however, the gains to literature in regard to quantity are almost equally great in all these branches of study, a similar assertion cannot be made with reference to the scientific results obtained. The Franco-German war created a new epoch in Military Surgery. The distribution of medical duties during and after engagements, the transport of the wounded, and military hygiene, these, in addition to the defining of a few general operative indications, have become the chief problems for mutual interchange of ideas, and for the much desired solution. It is only with regard to the principles of the treatment of wounds in war that, unfortunately, no agreement has been arrived at.

The second of these branches of study, which refers to the saving of blood in operations, initiated by the ingenious idea of Esmarch, shows at any rate an uniform attempt, based upon practical experience, and assisted at the same time by experimental tests of the facts obtained, permanently to secure for this method its modifying influence upon the practice of operations.

The case is otherwise with reference to the third subject of so many publications at the present time,—I refer to the doctrines with regard to Transfusion. To discover in this case any guiding principles is a task almost impossible. Canulæ, syringes,

* From a lecture delivered before the Obstetrical Society of Berlin, December 1st, 1874.

complicated apparatus, which were thought to be safely hidden away in venerable collections of ancient instruments, have again emerged from their hiding-places as new discoveries. Therapeutic ideas and indications, which, centuries ago, had been fought over and rejected as useless, are again discussed and defended. Innumerable experiments have been performed, but without any precise settling of the question, and without any strictly scientific method. Thus the strife rages in all directions, but it is a dispute in which no one is really vanquished; the combatants only elbow and tread upon one another. The casuistical literature of transfusion is therefore not the place where we can get any definite counsel with regard to this important operation. Permit me, therefore, in the first place, for a few moments to take you away from the bed-side into the experiment-room of a physiological laboratory. The experimental physiology of the blood will, I trust, give you a better answer to the many obscure questions in the study of the replacement and saving of blood, and may you also find in it a more certain footing for your medical treatment than in all the casuistry hitherto so prevalent in the science and art of therapeutical transfusion.

Let us begin with the consideration of a very simple experiment. We connect the jugular vein of a small dog with the carotid artery of a large one by a system of glass-canulæ and small caoutchouc tubes, in such a way that the arterial blood of the larger animal can flow directly into the jugular vein of the smaller one, and so that by applying the finger we can easily control the commencement of pulsations in the vein. Whereas the large dog is scarcely affected by the increasing loss, the little one at first resists the unwonted rapid increase in the quantity of his blood. But a change soon takes place. The animal which receives the blood becomes quiet, and its respiration, slow and superficial; on the other hand, the slight tremor of the muscles in the animal which is yielding the blood, shows us that the loss of this fluid from the artery is beginning to endanger the physiological condition of the organism. The tremor of the muscles increases to considerable restlessness, the animal whines, its respirations become deeper and deeper, and more and more frequent. In the meantime great changes have taken place in the small animal. The

eyes glisten and appear more prominent than usual, the conjunctiva looks more and more injected, the mucous membrane of the mouth and nose becomes distinctly red, and in places where the skin is thin, great vascular congestion becomes manifest. The abdomen is swollen, feels hard and tense and is sensitive to pressure. A copious discharge of fæces now usually takes place from the animal. Meanwhile the anæmia of the large dog goes on increasing. The cheeks are puffed out and drawn in by the respiratory current, the animal is no longer able to drag at his chain, and soon falls into general hæmorrhagic convulsions. We now stop the transfusion and first let the little dog loose. When thus set free he quietly trots away, and it is only by the tenesmus, which continues for some time to trouble him, that any change is indicated in his general condition. We direct, however, our attention in the first place to the dog which has lost the blood. The flow from the carotid has almost ceased, the blood comes away slowly, drop by drop. The breathing of the animal is scarcely perceptible, but the cornea is still sensitive. We therefore close the canula in the carotid, rapidly lower the dog's head, and at the same time raise its hind legs and rump as much as possible. Then we forcibly compress all four limbs in a centripetal direction in order to empty them as much as possible of blood, and we rub and knead the abdomen and thorax alternately. The result is a brilliant one. The animal begins to take a deep breath and to move itself more vigorously; we now open the canula in the carotid, and the blood again escapes in a pulsatory stream until death ensues from the hæmorrhage. Before we pass on to the explanation of these latter phenomena, it is time to examine carefully the condition of the surviving dog which has received an increase of blood. The animals had been weighed before and after the experiment, and the easily-reckoned loss from the fæces had been estimated, while in both animals the passage of urine during the experiment had been prevented by tying up the prepuce. If we reckon the normal quantity of blood in the dog at 7 per cent. of the weight of the body (E. F. G. Herbst, Comment. hist. crit., Göttingen, 1822, p. 59 *et seq.*, Heidenhain, Disquis. crit., etc., Halle, 1857, p. 36 *et seq.*), then our dog possessed before the transfusion about 294 grammes of blood, whilst about 570 additional grammes have been introduced into it from the

large dog. We have therefore imported into the dog almost twice as much blood as it normally possessed, without endangering its life or causing any direct injury to its general condition.*

This fact is of great importance, both in itself and in its consequences. The question then arises, where does the great increase in the quantity of the blood become lodged in the organism? Is there room enough within the closed vascular channels for so much blood, and, if so, what are the conditions of the arterial pressure? The first thought that would occur to us is, that when the supply is thus so greatly increased the

* The above experiment was performed on the 13th of November, 1874, in order that I might be able, when some time had elapsed, to bring before the Gynæcological Society a dog whose quantity of blood had been thus increased. Before the transfusion the dog had already been suffering from diarrhœa, which afterwards became considerably aggravated, but the stools never contained any blood. On the 14th of November the dog was quite lively; on the 16th showed great desire for food, and appeared to recover. However as time went on, under poor feeding and general want of care the dog became visibly thin, and, the wound on the neck being almost cicatrised, died on the 30th of November, seventeen days therefore after the transfusion, from increasing general weakness. The vessels of the conjunctiva had remained injected for fourteen days, whilst the extreme tension and sensitiveness of the abdomen had lasted for about ten days. The daily weighings of the animal are of no value because of the continuous diarrhœa. The post-mortem examination showed a relaxed condition of the heart, both sides of which organ were filled with dark clots of blood; a striking enlargement of the liver, which was abormally full of blood, so that on drawing the blade of the knife over the cut surface, a diffluent mass resembling the splenic pulp could be pressed out. Nothing abnormal in the other organs.

A similar course of experiments performed in the Physiological Institution at Leipzic yielded this result, that a dog into which a quantity of blood, at least equal to that which he originally possessed, has been introduced, may continue to live without any remarkable sign of disordered health. (L. Lesser, Ueber die Anpassung der Gefässe an grosse Blutmengen. Aus den Berichten der math.-phys. Classe der Kgl. sächs. Gesellschaft der Wissenschaften, 1874). Michel Rosa (Lettere fisiologiche, Napoli, 1783) had already satisfied himself by experiment that even a considerable increase in the quantity of a dog's blood had not been followed by any demonstrable surcharge of the vessels. Casse (De la transfusion du sang. Mém. prés. à l'académie de méd. de Belgique, le 23 November, 1873, ref. im Centralblatt für Chir., 1875, No. 15) found, in accordance with the above results, that the normal quantity of blood in an animal could be considerably increased, indeed doubled, without its health being thereby injured (*l. c.* p. 55).

blood would somewhere force its way through the walls of the vessels, and become effused into the surrounding tissues or into adjacent cavities of the body. Careful examinations of the bodies of these animals whose quantity of blood has been increased have been made by Worm-Müller (Die Abhängigkeit des arteriellen Drucks von der Blutmenge. Berichte der Kgl. sächs. Gesellschaft der Wissenschaften, 1873) and subsequently by myself, and with the exception of a few subserous ecchymoses as large as a pin's head (particularly after transfusion of defibrinated blood), neither of us was able to discover any escape of blood in any part of the animal's body. Only once, when, by injecting blood deprived of fibrine, I had increased the quantity in the animal's body, till it amounted to eighteen per cent of its weight, did I find a large quantity of blood extravasated in the intestine, in the mucous membrane and beneath the serous covering. But that either plasma or serum escapes in any considerable quantity from the vascular system, no one has ever been able to satisfy himself, either by catching the lymph as it flows from the thoracic duct while transfusion is going on, or by minute examination of any exudations that may possibly be present in the serous cavities and connective-tissue spaces. This is a proof that, even when a large quantity of blood is injected, by far the greater part remains within the vessels. It may be that principally the capillaries, the small veins, and the small arteries, are made available for the reception of the transfused blood, but it is also possible that the larger veins, at other times but slightly distended, may now contain a greater quantity. The former assumption is supported by a simple consideration of the store of blood and the capacity of the vascular channels in particular portions of the body. Both are subject to very considerable fluctuations, under various influences, without any deviation from the normal physiological activity being manifested by the parts. Paleness and redness, turgescence, swelling or relaxation are met with so often within physiological limits in the living organism, that it is not difficult to imagine that many vascular channels, containing but little or no blood, stand open to receive any that is introduced, and may be able, like a reservoir, to store up the additional quantity. We get however more definite information on these points

from observation of the condition of arterial pressure during transfusion.

In Prof. C. Ludwig's laboratory at Leipzig, Worm-Müller has discovered the very singular fact of the relative constancy of the arterial pressure upon increasing the quantity of blood in an animal.* If the normal quantity of blood be increased by two to four per cent. of the weight of the body, the blood-pressure certainly increases, but only in a slight degree and temporarily, and soon returns to its original height. If now the supply of blood be still further increased until the normal quantity is doubled or even tripled, then the pressure, which has again touched the normal, remains at its limit and cannot in any way be made to rise higher. In like manner when transfusion has been employed to animals that have been bled, the arterial pressure rises to its normal amount, but never beyond this, however much more blood may be added. Worm-Müller from this distinguishes, as it were, three ranges of the capacity of the vascular system. The first of these commences with the anæmia, and stops when the quantity of blood is less than the physiological amount in the animal in the natural state, by from 1.5 to 2.5 per cent of the weight of its body. In this case the blood-pressure rises permanently to the normal amount. The second range, following directly the preceding, includes a charging of the vascular system beyond the normal quantity by 2—3 or 4 per cent. of the weight of the body. Here the blood-pressure oscillates above and below its normal limits, the changes being evidently dependent upon the regulating influence of the vaso-motor nervous system, which allows the vessels to become adapted to an increase of their contents up to a certain point. For us this division has an especial interest, because it includes those quantities of blood which may come within the range of our transfusion operations for therapeutical purposes. But the third division also appears to be especially worthy of notice; the facts with regard to it have been really already anticipated both by the experiment and by the previous considerations. Here, probably in consequence of a stretching of the walls of the vessels, the pressure remains without altera-

* *Conf.* Joh. Jac. Woltersom, Spec. physiol. med. de mutat. in sano corpore sanguin. detract. prod. Diss. Arnheim, 1850. *Conf.* Majendie, Phenomènes physiques de la vie. Paris, 1839. T. iii. p. 89 *et seq.* and Spengler, Müller's Archiv. 1844, p. 69.

tion, at its normal amount, even after the introduction of enormous quantities of blood. But, as you have seen, there is, in all probability, no danger to life even when the vessels are thus so greatly surcharged. The exploration of this, in one direction unbounded, field is replete with experimental tasks which will amply repay further observers. We, however, will content ourselves with endeavouring to accommodate the above important facts to the views hitherto customarily held as to the quantity and pressure of the blood. In the first place it is clear that the fears, so often entertained and expressed that the blood-pressure in the human subject could by therapeutical transfusion be so increased as to cause permanent danger, are only relatively justified, and, as a general rule, are devoid of any foundation in fact. Such fears might perhaps be justifiable in cases of chronic anæmia in which the vessels are permanently less distended, and pathological changes in the circulatory apparatus may be supposed or proved,* and likewise in cases where there are such diseases of individual organs that even increased vascular pressure, as produced by mental impressions through reflex irritation of the vaso-motor nerve-centres, would cause dangerous symptoms. This first category we must, however, include under what W. Müller describes as the first degree. But here the blood-pressure would at most rise permanently only to the same point as it was when the patient was in a healthy state. The second category, which accommodates itself best to the second degree, shows us the critical point in every increase, even though temporary, of blood-pressure above what is normal. This, however, may be produced not only by an increase in the quantity of blood, but by blood-letting to a slight amount, so that in this respect two entirely opposite causes appear to produce the same effect. In the acute anæmia of healthy persons the above considerations do not at all apply, we shall therefore once for all dismiss the still so frequently defended opinion that venesection and depletion must always precede copious transfusion, and must maintain that depletion is only admissible in cases where some substances, poisonous to the organism, have gained entrance into the blood, and can be extruded from the body along with the blood, and in no

* Perl, Ueber den Einfluss der Anämie auf die Ernährung des Herzmuskels (Virch. Archiv. Bd. 59).

other way. A fate, very similar to that of transfusion, fell to the share of Esmarch's method for avoiding loss of blood, for there was a dread lest by bandaging the limb to be amputated, and by methodically pressing out the blood from a sanguineous tumour about to be extirpated, the remaining blood-passages in the body should be filled to over-distension. The error of this idea has been clearly demonstrated by the abundant experiences of this method in practice, experiences which are quite in harmony with our physiological results. Only Jefremoff,* Chauvel,† and after him, Augier,‡ assert in point of fact (the first-mentioned from his experiments on dogs) that they discovered an increase of the blood-pressure after bandaging the limbs. We can, however, readily account for Augier's results, from his own statement that the observations were made upon anæmic individuals. In these cases there must certainly be an increase of blood-pressure, not however going beyond, but only up to, the normal amount, as we shall understand more perfectly by-and-by.

With regard to the group of symptoms observed during transfusion, we will, by way of supplement, add that in the physiological experiment, even when the vascular system is greatly overcharged, no change can be demonstrated in the heart's capacity for action (number of pulsations, increase of blood-pressure when the supply of blood to the right side of the heart is momentarily increased), neither do we find that there is any engorgement of the lungs.§ This latter circumstance is certainly of interest with reference to the suggestions, nowadays from time to time in favour, for injections of blood into the veins of phthisical patients, and its importance is only the more confirmed by the fact that hæmorrhages from the lung have been very rarely reported after such transfusion, whereas, on the other hand, escape of blood into the bowel has been not

* Jefremoff, Künstliche Blutleere durch elastische Binden. Inaug. Diss., St. Petersburg, 1874 (*cf.* Centralblatt f. Chir., 1874, No. 23). He found that in a dog, after bandaging up to the middle of the femur and humerus respectively, the quantity of blood of the lower extremity amounted to a thirty-fifth part, that of the upper extremity to a twenty-sixth (!), of the total amount in the body.

† Chauvel (Union médicale, 17 Nov., 1874).

‡ Augier, De l'anémie artificielle dans les opérations des membres. Thèse, Paris, 1874.

§ *Conf.* Casse (*l. c.* p. 55).

unfrequently noticed. Generally speaking, that engorgement with blood which occurs after copious injections, in the mucous membranes which are visible (the conjunctiva, the mucous membrane of the mouth and nose), as well as in the veins of the skin, especially on the abdomen, as also the great tension of this latter part, must mainly be regarded as the consequences of a stagnation of blood in a backward direction from the venæ cavæ. It has not as yet been positively proved by experiment whether, in the abdomen, under rapid injection into the jugular vein there is, to any extent, a direct engorgement of the portal system as far as the venous radicles in the intestinal mucous membrane, etc. But the tenesmus which often lasts for hours after transfusion, the frequent vomiting of mucus tinged with blood, as well as the diarrhœic evacuations* frequently mixed with blood, all of which symptoms occur in experiments just as regularly as in patients, require at any rate, that, in addition to carefully examining the heart and lungs, we should before everything else give greater attention than heretofore to the state of the intestinal mucous membrane and of the large abdominal organs.

The results of bleeding the animals that had been surcharged with blood, yield perhaps a more significant explanation with regard to the quantity of blood, and its distribution in the animal economy. According to Heidenhain (*l. c.*), Panum

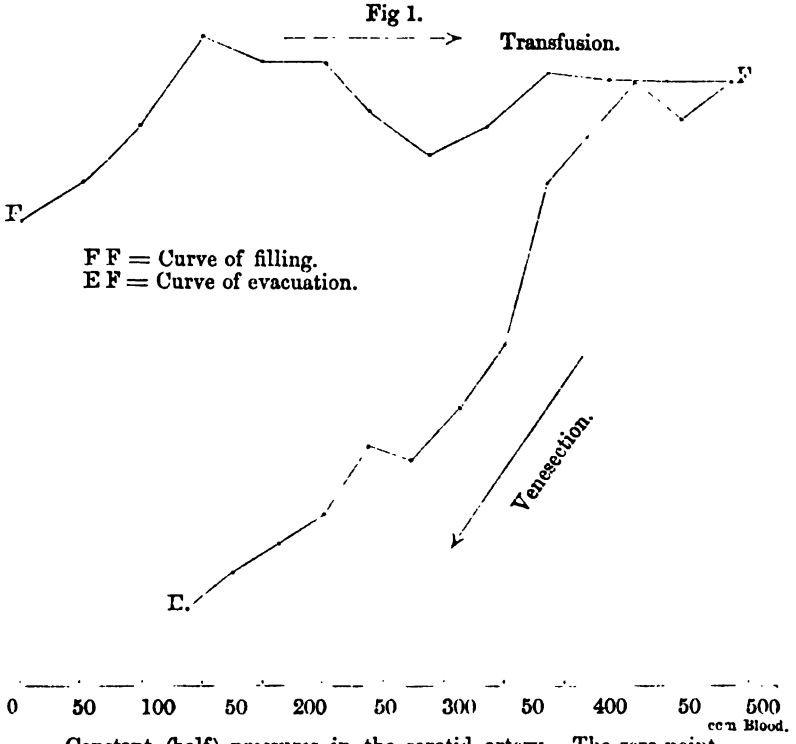
* In the experiment related on a previous page (see the account of the post-mortem examination) the abnormal enlargement of the liver, and the unusually large quantity of blood contained in that organ are particularly striking phenomena. An experiment of the kind by Casse (*l. c.* p. 54) is also very remarkable. This experimenter, by means of Dieulafoy's aspirator, conveyed from the jugular vein of a large dog 375 grammes of entire blood into the left saphenous vein of another dog weighing 4,700 grammes. The operation was very quickly performed in order to avoid coagulation. The dog moaned a little and vomited, and soon fell into a state of syncope. Attempts at restoration were unsuccessful. On examination made a quarter of an hour afterwards a large amount of blood was found free in the abdominal cavity. The intestines and the omentum were normal; none of the other organs were distended with blood, the brain was even anæmic. The liver only showed very numerous fissures in many places, and a striking enlargement of all the lobes. On section a considerable quantity of blood escaped from the cut surfaces.

The examination of the vessels in the bones, and of the venous plexus in the vertebral canal has not as yet been sufficiently attended to.

(Virch. Archiv. vol. 27, parts 3 and 4), Tappeiner, W. Müller (*l. c.*), and others, the tension of the contents of the vessels sinks to zero, when blood is removed from dogs in a proportion of 4, 5, up to 6 per cent. of the weight of the body. If, before the venesection, the animals are injected with a large quantity of blood, we should expect that when they were bled to death they would yield considerably more blood than in a normal condition, for at least a portion of what has been transfused would, we should suppose, be added to the 4, 5, or 6 per cent. of the weight of the body. This, however, is not the case. The investigations by W. Müller and myself have shown, that even from animals which have been made the subjects of transfusion, and afterwards been bled to death, it almost always happens that only the above-mentioned percentage of blood can be obtained, and that, generally speaking, we are not even able to get back as much blood as we have injected; the dogs usually die with a quantity of blood remaining in them, several hundred grammes in excess of the normal amount. This apparent paradox of a fatal hæmorrhage, with an abnormal amount of blood remaining, becomes intelligible to us on a consideration of the respective conditions of blood-pressure. The amount of blood contained remaining the same, there are great differences in the arterial pressure, according as this amount is obtained by artificially increasing the original quantity of blood of a normal animal, or by withdrawing blood from an animal whose vessels have been surcharged. An instructive diagram which we find in W. Müller (*l. c.* p. 637) will make this clear to us. The blood-pressure gradually rises during the injection of the first 120 ccm. of (defibrinated) blood, and then generally remains unaltered, notwithstanding an additional injection of 350 ccm. (making a total of 470 ccm.) On withdrawing the blood, the first 100 ccm. produce but a slight effect upon the arterial tension; from thenceforward, however, the blood-pressure rapidly and suddenly falls, and when only about three-quarters of the quantity injected have escaped, that is, when there is still a surplus of 110 ccm. above the normal quantity, the pressure becomes so low as to indicate great danger to life. We therefore see that animals whose bodies have been surcharged with blood become, as it were, more sensitive to losses of this fluid; they require much larger quantities of blood than

normal animals, in order to keep the blood-pressure at a height necessary for the continuance of life. I abstain here from going more minutely into certain analogies with the hæmorrhages in pregnant and recently-delivered women, the more so because at the present time, so far as I know, accurate determinations as to the quantity of blood in non-pregnant and pregnant women, and in those who are past child-bearing, are

Fig 1.




Constant (half) pressures in the carotid artery. The zero point marks the normal quantity of blood.

wanting, just as is the case with regard to comparative physiological investigation (in bitches for instance) in this direction. Inasmuch, however, as the blood-pressure often suddenly falls even during the withdrawal of blood in normal animals, it is evident that even in blood-letting under ordinary circumstances, the quantity of blood to be withdrawn must be accurately measured in order that the depletion may never be followed by a sudden threatening collapse.

Are there any means at our disposal, are we able, by operative measures or otherwise, successfully to encounter such emergencies? Here also, the simple experiment which we made at the commencement of our investigations affords us a satisfactory basis.

It is a fact that an animal dies much sooner from hæmorrhage and yields, therefore, a smaller quantity of blood if he is placed and kept fixed in a horizontal position. The fatal collapse will be still further accelerated if we raise the head of the bleeding animal, and allow the hinder-parts to fall down.* On the other hand, in animals which have lost a great quantity of blood, life may be prolonged for hours by simply keeping them upon an inclined plane with the head downwards.† Much more lasting in their effects and often followed by permanent elevation of blood-pressure until life is safe, are shampooing manipulations of the extremities (or pressing out the blood by applying bandages, *e.g.* Esmarch's elastic ones), kneading the abdomen and compressing the thorax. The index of the kymograph marks distinctly the rapid elevation of the arterial pressure which had fallen so low;‡ the almost exhausted current of blood begins to flow more vigorously from the depletion-canula, so that a very considerable quantity may still be obtained.§ It is moreover very remarkable that animals previously surcharged with blood, and subjected to the above manipulations, exhibit not only a much higher degree of blood-pressure, but also yield a much larger quantity of blood than is the case with normal animals. The more sanguineous therefore the individual, and the earlier the occurrence in such a one of the symptoms of collapse after loss of blood, the greater the chances that the employment of the above-mentioned manipulations will enable us permanently to remove the dangerous symptoms. In the absence of such manipulations, patients

* Pozzi (Progrès Médical, 1874, Nos. 40 and 41) asserts that he has found that death occurs after a smaller loss of blood from the carotid than from any other artery—the femoral, for instance.

† *Conf.* the results obtained by Irschik on the external jugular vein of dogs. (Inaug. Diss., Dorpat, 1874, ref. to in Centralbl. f. Chir., 1874. No. 11.)

‡ This, as well as the other changes of blood-pressure already described, was demonstrated to the Gynæcological Society of Berlin by the original curves. It would be inconvenient to reproduce these on this occasion.

§ Jefremoff (*l. c.*).

would certainly die, not from want of blood, but from its defective distribution. As soon as arterial pressure sinks while loss of blood is going on, the *vis a tergo* which favours the escape of blood towards the heart also becomes less. It would be thought too teleological if we were to connect the convulsions which occur in hæmorrhage, and the falling back of the head, with an instinctive effort on the part of the organism towards a spontaneous increase of blood-pressure and an increased supply of blood to the heart or brain. But during venesection the blood at any rate becomes stagnated always to a greater extent in those parts which are no longer influenced by the propulsive force of the heart. Thus when the loss of blood is very considerable, the effort of nature to arrest the hæmorrhage becomes a cause of death. The individuals die not from want of blood, but from want of movement of that fluid.

You see, Gentlemen, what widespread importance is already attached to our present experiments and observations, what a reforming influence they cannot fail to exercise over all our therapeutical efforts in loss of blood and its consequences. Strange to say, the method of autotransfusion, as it has been happily named by the French, has, notwithstanding its simplicity, failed to obtain in practice the reception it deserves. Prof. P. Müller, from Würzburg (at present of Berne), was the first to propose the methodical bandaging of the limbs according to Esmarch's process, in cases of great loss of blood after delivery.* From various quarters we have had observations of the favourable results of the above treatment; recently, for example, in Billroth's hands after great loss of blood during the removal of an inter-parietal fibroma of the abdominal walls.† There have also been many cases of ovariotomy in which the threatening collapse produced by great loss of blood has been warded off by autotransfusion. It is therefore more incumbent upon us to give particular attention to the subject of autotransfusion, the scientific foundation of which we have so clearly

* I am indebted to the kindness of a distinguished obstetrician for the information, that in western and southern Germany the application of bandages to the limbs has been for a long time employed as a popular remedy by midwives in profuse hæmorrhages after delivery.

† Wiener med. Wochenschrift, 1874, No. 36, *cf.* Centralblatt f. Chir., 1874, p. 527.—*Cf.* also Billroth, chirurgische Zeitfragen (Transfusion, Wiener med. Wochenschr., 1875, No. 2.)

seen. By laying down definite indications we shall be able to save many a life in jeopardy, or even considered lost, cases for which we formerly had no remedy, whether they occurred in child-bed, on the operation-table, or in the battle-field. The careful employment of autotransfusion will, on the other hand, restrict the field for the operation of transfusion; but only by excluding doubtful results and by putting aside many cases unsuccessful because the attempts were scientifically irrational, can we secure for transfusion the high rank which it deserves to hold among operations for saving life.

The indications for autotransfusion must for the present be stated as follows:

1. Loss of a moderate quantity of blood, followed by sudden symptoms of collapse hitherto often regarded as inexplicable. In this category are included a great number of cases in which results are said to have been obtained by injecting even very minute quantities of blood. Inasmuch as in these cases the threatening symptoms very often disappear spontaneously when the interruption to the circulation has been repaired, and as the use of mechanical and medicinal excitants has very frequently proved successful, it is at least doubtful whether, under such circumstances, we are justified in having recourse to operative transfusion. In cases, therefore, similar to those in which the injection of small quantities of blood formerly proved successful, we shall very reasonably substitute for transfusion what is now called autotransfusion, this most powerful of all remedies termed excitants, that is, those which increase blood-pressure. In other cases, autotransfusion, as we shall see by-and-by, appears at any rate to be an experiment which may be tried before every operation of transfusion. Further experience is required to teach us whether autotransfusion will prove itself to be a directly hæmostatic remedy for hæmorrhages otherwise uncontrollable, as for example those of the uterus, and whether it will act in this respect like direct injection of blood, namely, by exciting the contractions of the unstriped muscular fibres, such as those of the uterus.

2. In anæmic cases, both before and after operations in which a fresh loss of blood is unavoidable, but would be attended with dangerous consequences to patients of this kind.

3. In anæmic persons, about to undergo operations, in

whom we have to decide as to the exhibition or prolonged exhibition of chloroform. According to the statements of Lenz, Brunner, Gall, Scheinesson,* during the inhalation of chloroform, there is a constant and considerable decrease in the blood-pressure, principally due to paralysis of the vaso-motor centres. Hence it naturally follows that it is of great importance in anæmic cases in the first place to increase the blood-pressure by autotransfusion, before the inhalation of a remedy which has such a powerful effect in the opposite direction.† On the other hand, our experiences with regard to autotransfusion will in great measure enable us to explain the effect, in chloroform-asphyxia, of the recently recommended inversion, or of Nélaton's plan of lowering of the head, without compelling us to have recourse to other ingenious hypotheses, where we have experimental demonstration.

4. In the last place, autotransfusion is worthy of special consideration as a preliminary step to transfusion when this latter is really indicated by profuse hæmorrhage. The performance of autotransfusion in the interval, hitherto unemployed and yet so valuable as regards life, which is consumed in the necessary preparations for transfusion, will in many cases be the only means of ensuring the success of this latter operation, and will often spare us the dreadful words "too late." The employment of this method is the more easy, as the necessary manipulations can be entrusted to any one, however previously inexperienced.

But not only as regards therapeutics, but also for the purposes of diagnosis does autotransfusion render us essential service. We have indeed seen that the arterial tension alone is by no means an indication of the quantity of blood present in an individual. On the other hand there is no method so good as autotransfusion for giving such definite information with regard to the quantity of blood at the disposal of the organism. If its effect is found to be great as contrasted with the intensity of the collapse, we shall then willingly avoid having recourse to any importation of blood from without. Supposing that the effect is less, and transfusion becomes abso-

* *Conf.* die Literatur bei W. Kock, Ueber das Chloroform und seine Anwendung in der Chirurgie (Sammlung klin. Vorträge von R. Volkmann, No. 80). † *See* Augier also (*l. c.*).

lutely necessary, the influence of the autotransfusion on the blood-pressure will at any rate be a better index of the quantity of blood to be introduced than the pulse by itself and all the symptoms of collapse, which, from what we have just seen, may be very pronounced even in persons who have still a good supply of blood in them.

In accordance with this we may make three divisions of anæmia analogous to the three degrees of blood-profusion, which have been adopted by W. Müller. Thus we distinguish :

1. Transient anæmia. This is produced by slight losses of blood, 1.5 to 2.5 per cent. of the weight of the body. In this the blood-pressure falls but little, in fact it even rises temporarily in consequence of reflex excitation of the vaso-motor centres; generally speaking it oscillates between the normal and its subsequent level until the latter is reached, generally spontaneously, by simple adjustment of the distribution of the blood in the body. This may be termed the physiological stage of anæmia.

2. Dangerous anæmia, following greater losses of blood, up to 3 per cent. of the weight of the body in normal individuals; in such as are abnormally full of blood this anæmia may commence even when the store of blood is 3 per cent. above the normal. Spontaneous adjustment very rarely takes place. But this condition can often be converted into the first degree by pressing out the blood from the extremities, abdomen, and thorax. This is the proper field for autotransfusion. It is only in certain cases, and then preferably in normal individuals, and therefore in those with an abnormally copious blood-supply, that we shall be compelled to have recourse to transfusion proper.

3. Fatal anæmia. The blood-pressure can no longer be raised by autotransfusion to the height requisite for life because of the gradual cessation of the heart's contractions. In a case of this kind, operative transfusion performed at the right time while the heart still beats,* and assisted by previous autotransfusion, will preserve life.

Before, Gentlemen, we leave the experiment-table, from which I trust you do not part without a certain feeling of satisfaction at what you have seen, we must, although very briefly, define our position with regard to the two principal questions

* *Cf.* Böhm, Centralblatt für die med. Wissensch., 1874, No. 21.

in the modern doctrines of transfusion. These are, firstly, whether the direct transfusion from person to person is to be preferred to the indirect method in which instruments are used, and in the second place whether we should use defibrinated or non-defibrinated blood ("ganzes Blut," Gesellius). The violence of the strife which has raged on these questions has fortunately not only invested them with the importance of principles, but has elevated these into dogmas, a sufficient reason for preventing all impartial persons from recognising their validity. In our experiment with the two dogs you have seen the blood conducted from the artery of the blood-giver to the vein of the blood-receiver, and you have witnessed at the same time the ease and safety of this procedure. A great number of objections have been raised against this method, especially with regard to the dangers of coagulation and the entrance of air, indeed only recently the attempt has been made to refer the symptoms observed in direct transfusion (from animal to animal) to the influx of arterial blood under great pressure into non-physiological channels.* At any rate the above method has, both in the hands of experimenters and also when adopted for therapeutical transfusion, proved itself to be the best, and this not only in recent but even in early times (Denis and Emmerez†) And we also, when the operation was quickly performed, never had reason to complain of any untoward complication of the kind indicated. In like manner, for simplicity and cleanliness of application, we must give the preference to glass canulæ, fitted with caoutchouc tubes, before all the transfusion apparatus still so much extolled.‡ With regard to the operative procedure itself, the statements made on that subject by Hasse at the third Surgical Congress at Berlin (*see* "The Transactions," p. 110), deserve the fullest recognition. I should like to draw

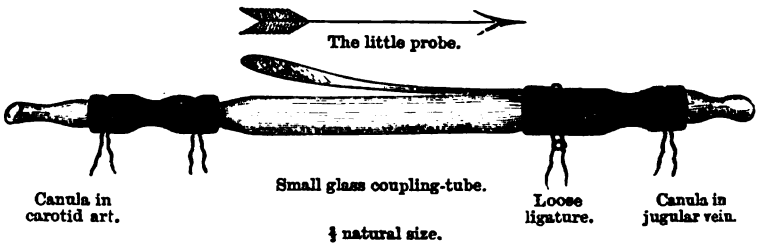
* Heyfelder, Zur Lehre von der Transfusion. Deutsche Zeitschr. für Chir., vol. iv. part 4.

† Libavius (Appendix necess. synagmat. arcanarum chym. Francof. 1615). "Adsit juvenis, robustus, sanus, sanguine spirituoso plenus; adstet exhaustis viribus, tenuis, macilentus, vix animam trahens. Magister artis habeat tubulos argenteos, inter se congruentes, aperiat arteriam robusti et tubulum fœmineum infingat, jam duos tubulos sibi mutuo applicet, et ex sano sanguis arterialis, calens et spirituosus saliet in ægrotum, imoque vitæ fontem afferret, omnemque languorem pellet."

‡ *Cfr.* Casse, *l.c.*

your attention to a small contrivance which does away with the necessity of previously filling the canula in the vein with a solution of soda (of a quarter per cent. strength) in order to prevent the admission of air. After fixing the canulæ in the artery and vein you immediately unite them by means of a glass tube which may be more or less bent at its middle as circumstances may require.

Fig. 2.




Between the glass tube and the raised-up caoutchouc-sheath of the canula in the vein, you introduce a metallic probe until it freely projects within the sheath. Whilst now the vein is being continuously compressed (this is best done with the fingers), you raise the small holdfast forceps at the central end of the artery, the blood immediately rushes into the little apparatus, driving before it the air which escapes externally by the side of the probe. When the first drop of blood unmixed with air-bubbles escapes by the side of the probe, this latter is rapidly withdrawn and compression of the vein is discontinued, the blood then flows freely and unchecked into the organism which requires it. There is but little to remark with regard to other methods, as we have too little experience of them. Interesting as are the transfusions, for example, from artery to artery and from vein to vein, they all come at present under the category of the indirect transmission of blood, inasmuch as they are performed by the aid of pump apparatus. It is certainly true that these constantly improved instruments, which, however, are almost universally constructed on the model of Dieulafoy's aspirator, have opened a still larger field for the direct transfusion of entire blood as opposed to the injection of blood that has been defibrinated, but in spite of all the praise that has been bestowed upon them they will scarcely be able to obtain a permanent place among our means for transfusion. The indi-

rect transfusion of defibrinated blood was, in our experiments, not performed with a syringe, but under constant pressure of a column of mercury for which the corresponding apparatus of two communicating glass flasks filled with mercury can be easily arranged, and then connected in a convenient way with the vessel containing the blood. It is unnecessary to give a more minute description of the process, which is exactly analogous to that adopted for the artificial injection of organs with colouring matters for microscopical purposes. In indirect transfusion for therapeutical purposes, the inconvenience connected with a syringe is best avoided by using an open glass vessel with a caoutchouc tube, from which, as from a wound irrigator, the blood flows steadily and quietly into the vein under the pressure of its own column of fluid. Primitive in their form and generally capable of being extemporised, these contrivances have done better service than any others in the practice of transfusion (*conf.* Casse* and Braxton Hicks†), and this too with an utter absence of such accidents as are reported to have occurred under the use of instrumental pressure.‡ Besides this I would especially draw your attention to the uselessness of previously warming the blood to be transfused until the temperature of the body is obtained; this holds good of the injection both of entire and of defibrinated blood. On the one hand, according to the experiments of N. Duranty,§ Oré,‖ and Casse,¶ the coagulation of the fibrine is materially retarded by considerably reducing the temperature of the blood, and, on the other hand, the above named observers, with whom Malgaigne** and Polli†† agree, have never seen any evil consequences

* *l. c.*

† Braxton Hicks, Cases of Transfusion, etc. Guy's Hosp. Rep., 1869, No. 5, xiv. pp. 1-14.

‡ Eulenburg und Landois, Die Transfusion des Blutes. Berlin, 1866, p. 57 *et seq.*

Wilh. Löwenthal (Heidelberg), Ein Beitrag zur Lehre von der Transf. des Blutes. (Apparat von Belina.) Berl. klin. Wochenschr., 1871, No. 41.

§ N. Duranty, Thèse de Paris, 1860.

‖ Oré, Gaz. des Hôpitaux, 1865, 30 décembre.

¶ Casse (*l. c.*)

** Malgaigne, Traité d'anat. chirurg. 2 édit. vol. i. p. 480.

†† G. Polli, Glorie e sventure della transfusione, Annali universali de medicina, 1854, Mars.

when the temperature of the injected blood was only equal to that of the air in the room. It is, however, this warming of the blood that causes indirect transfusion to be a troublesome and tedious process, and it is therefore to be hoped that the superstitious notion that the blood for transfusion must be of the same temperature as that of the body will soon disappear from handbooks and from the minds of physicians.

In direct transfusion of blood from individual to individual there has been hitherto great inconvenience, as compared with the transfusion of defibrinated blood, in measuring the quantity to be introduced, and secondly, in determining the pressure at which the influx was to take place. The determination of both data, however, is a scientific consideration, demanded also in therapeutical transfusions, if the statistics of these latter are to be of any use for the future. The method devised by Hasse* is a very inexpedient one; he uses a sheep, and after the transfusion is finished he collects some more blood from the carotid in a cylindrical measure, and estimates the amount that has been transfused from the amount which escapes in a unit of time. The hæmorrhage-curves, as they have been expressed by Tappeiner, Slavjansky,† and others, will be sufficient to convince Hasse of the physiological errors involved in his proposal. The fact is that the only way to obtain really useful results is to determine the weight of the body before and after the transfusion, as we have done also in our experiment. Spring balances are a good kind of appliance for determining the weights, a contrivance for suspending the animal to be used being substituted for the scale pan. Male animals are more convenient for manipulation, because the loss of urine can be prevented by placing a ligature for the time round the prepuce; the plan we also adopted. It is not difficult to estimate the loss from fæces. In my opinion it is only in clinical wards where ample assistance can be obtained and where we can take plenty of time and everything is quiet, that the determination of the blood-pressure can be so made as to be valuable and trustworthy. Under such conditions, however, before introducing the artery-canula into the vein-canula of the patient, it is easy

* Hasse, Die Lammbluttransfusionen beim Menschen. St. Petersburg, 1874.

† Arbeiten der phys. Anstalt zu Leipzig.

to connect the former with a steady and well-made mercurial manometer, and to make the necessary readings.

We now, in conclusion, pass on to the second of our principal questions, and with regard to this we can thoroughly confirm the account of the symptoms occurring after the injection of defibrinated blood given by Panum, Mittler, Gesellius, Hasse, W. Müller, and others. The more frequent occurrence of ecchymoses and extravasations after the injection of defibrinated blood, the parenchymatous hæmorrhages of mucous membranes which are therefore seen, and (a point of especial importance) from wounds existing or established (*cf.* the results of Taburé's experiments*), and finally, as observed by W. Müller and others, the reddish colour acquired by the lymph, in addition to the retarded coagulation and the diminished contractile power of the crassamentum with further repeated separation of fibrine in the serum which generally contains hæmatine; all these phenomena indicate that transfusion of defibrinated blood is at all events an unwarrantable method for cases of recent wounds, and also where our object is, by injecting blood, to check hæmorrhages otherwise uncontrollable. With this we must also connect the experiences of Mittler and Hasse, which are to the effect that the organism tolerates, and without detriment, large quantities of natural blood better than equally large quantities of defibrinated blood. In harmony with this are the warnings of Panum,† so competent an observer in this field, against overcharging the vessels with blood deprived of its fibrine. It is therefore in cases in which it is contemplated to introduce large quantities of blood, that entire blood is to be preferred to that from which the fibrine has been removed. We have hitherto tacitly assumed the transmission of the blood from one individual to another of the same species. The conditions are more complicated in the transfusion of blood from animals to men, as also in the experiments for interchanging blood between animals of different species. In addition to the symptoms hitherto described, which are no doubt chiefly to be referred to quantitative changes in the composition of the blood, there are also the chemical influences of the individual constituents of the blood-donor upon the blood and tissue of the

* Taburé, Diss., Petersburg, 1874, ref. to in Centralbl. f. Chir., 1874, No. 1. p. 6. † *l. c.*

blood-receiver; the effects of these influences appear to be partly unknown and in great measure still unexplained. By way of examples we may cite the controversies with regard to the subsequent destiny of the red blood-disks of one individual in the circulation of another (Gesellius, Hasse, Ponfick, Sanders, and others); also Landois' minute investigations with regard to the effect of the serum of different species of animals upon the blood-disks of the blood-receiver,* and then too we may allude to a point which has lately been properly insisted upon, viz. the importance of the gases of the blood, and the different quantities of such gases in different kinds of blood.†

You will see, Gentlemen, from what has gone before, that neither our experimental investigations, nor practical experience up to the present time, can afford a sufficient answer to the two propositions which we have termed cardinal questions. It will, however, never be possible to give an answer unless the question be put in a different way. In fact it is not altogether a question as to whether we are to give the preference to human or to animal blood, to blood in the natural state or to blood defibrinated, but the point is rather to establish whether we may in special cases inject human or animal blood, and with or without fibrine. Transfusion, like tracheotomy, is and will be, above all things, a means for saving life. Its real powers in chronic cases of disease will only be discovered when the results obtained are tested with absolute scientific calmness. Transfusion, however, will always prove itself to be a most certain auxiliary when we have to deal with acute conditions dangerous to life, and these conditions are either bloodlessness or asphyxia of the tissues. In other words, we have principally to employ transfusion, first, for hæmorrhage which is otherwise uncontrollable, and its consequences, and, secondly, where such changes have taken place in the composition of the blood as to render it no longer able to convey to the tissues the materials all-important for their vitality. From the above considerations it must be evident that in dealing with hæmorrhages and their consequences, the only rational course in transfusion is to employ *entire* blood, whereas in cases where the blood has become

* Centralblatt für med. Wissensch., 1874, No. 24.

† L. Traube, *conf.* Küster, Ueber die directe arterielle Thierblut-Transfusion. Arch. für klin. Chir. xvii., iii. Heft.

incapable of effecting the interchange of its gaseous constituents, the especial point is the conveyance of blood, arterial or capable of becoming so, no matter whether it contains fibrine or not. Entire human blood is, beyond all question, the best material for transfusion for the human subject. It is true, however, that the applicability of defibrinated human blood to a number of cases cannot be disputed. It is only as a secondary consideration that we may inquire whether we may be justified in employing the blood of animals as a substitute for that of men. Inasmuch as according to our investigations with regard to blood-pressure, for the instantaneous effect of transfusion in losses of blood it is principally mechanical, and not chemical considerations that we have to think of, it may sometimes happen that no objection can be raised against the employment of entire animal blood in cases of acute anæmia, provided that the blood to be transfused has no poisonous effect upon the blood-cells and tissue-cells of the blood-receiver. It is only to be wished, in the interests of "bleeding" humanity, that on this subject of the transfusion of animal blood all the most comprehensive experiences should be collected, and that, above all things, other animals should be discovered which could be more easily and rapidly obtained in large towns than the sheep, hitherto almost the only animal which has furnished the blood for transfusion in the human subject. Gesellius* has proposed, though only in a cursory manner, that the dog should be tried as a source of blood, and the proposition is well worthy of consideration.

We are now at the end of our experimental enterprise, which has explained to you many questions which could not at present be answered at the bed-side. There is still, however, a vast field for further inquiry—based upon the facts we have obtained. I must abstain from discussing purely physiological points, the further cultivation of which will be associated with the fact that animals can be kept alive after having been very much overcharged with blood. But I have already reminded you of some, though only probable, analogies with the female organism in a state of pregnancy. The fact that we are able

* Gesellius, Zur Thierbluttransfusion beim Menschen. St. Petersburg, 1874.

to produce plethora in animals by artificial means gives us a handle whereby we may test experimentally all the abundant literature relating to plethora in the human subject. It promises also interesting conclusions with regard to the modifications in the metamorphosis of tissue in plethoric animals, and likewise with regard to the period of time during which an animal retains the excessive quantity of blood, that is, in how short a time from salivation, discharges from the bowels and urine, the quantity of blood may reach the normal, presumably with corresponding concentration of the serum.*

Furnished with the views thus obtained with regard to the capacity of the vascular system, and the corresponding conditions of blood-pressure, we now, Gentlemen, return to the bed-side. The general notions with regard to fullness of blood and anæmia will for the future appear to you in a new light. You will discover for yourselves more definitely and precisely what are the indications for artificially supplying blood. And above all things, in autotransfusion you have become acquainted with an important auxiliary to, and in very many cases a worthy substitute for, the operation of transfusion. At the bed-side of a bleeding patient instead of wasting valuable time in exhibiting internal stimuli or applying remedies of a similar kind to the skin, you will in preference have recourse at once to autotransfusion, this most rational and most efficacious of all manipulations for increasing the blood-pressure, and in cases where the stream of life is only apparently exhausted, you will with a steady hand propel it into channels where its presence is essential.

* Interesting in this respect is the history of a patient, a young woman who had become anæmic from uterine hæmorrhage, and into whom Bélhier (Gaz. des hôp., 1874, No. 31) transfused 80 grammes of entire human blood with Moncoq's apparatus (Transfusion instantanée du sang. Paris, 1874. Referred to in Centralbl. für Chir., 1875, No. 11). The estimation of the number of the blood-corpuscles according to Malassez's method (Arch. de phys. norm. et path. Janvier, 1874, No. 1), established the fact of an immediate numerical increase, then during the fever a diminution (probably in consequence of a relative increase of the colourless cells), and finally, even after eight days, an increase in the number of the red blood-cells, amounting to almost three times the normal quantity.

THE

# HÆMORRHAGIC INFARCTION OF THE LUNGS.

BY

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GENTLEMEN,—You had the opportunity during last session of seeing a considerable number of cases of hæmorrhagic infarction of the lung on the post-mortem table, and these cases were specially worthy of attention, because they had furnished at the bed-side well-marked symptoms of this affection. We observe these infarctions in cases of disease of the heart mostly, where their appearance usually introduces the last phase of a long illness, and often leads to one of the darkest scenes of human suffering. We may be tempted to look upon the infarction as a *modus moriendi* of heart cases, just as certain forms of pneumonia regularly develope in cases of slow suffocative death. It would almost seem to me as if these infarctions, which project on the surface of the lungs at the post-mortem in cases of heart disease, and can hardly be overlooked in the diagnosis, often receive too exclusive a consideration in the description of the disease. There are not a few anatomical results, and certain convincing clinical observations which point to cured or curing infarctions in cases of heart disease. But if we leave out of consideration heart disease, and if we pass over pyæmia, we meet with the infarction, somewhat more rarely indeed, yet frequently enough to recognise it as a comparatively mild disease, and one which under favourable circumstances is eminently curable. We will base our remarks on the certainly established anatomical results which last session furnished us with, in order to acquire safe rules by which to judge of other curable cases, and to indicate to us the conditions of their cure.

Lænnec* designated the new disease which he described, apoplexy of the lungs. In this name he looked exclusively to the analogy with apoplexies arising from cerebral hæmorrhage from the rupture of diseased vessels. In the same sense Dittrich† afterwards brought the infarction into relation to disease of the wall of the pulmonary artery. This original view of Lænnec has to this day some adherents.

On the whole, it is now considered more correct to compare the pulmonary apoplexy of Lænnec with those other lesions in the brain which also produce apoplectic symptoms, and which are usually included under the name "red softening." They are generally caused by the passage into the cerebral arteries of plugs formed elsewhere (embolic apoplexy) much more rarely from clots formed on the spot (autochthonic).

Even Lænnec encountered dry, firm clots in the neighbourhood of the infarction. He thought they had their seat in the veins. Since then they have played a very important part in the history of the infarction, but all later observers have found them much more frequently in the arteries. Bockdalek‡ considered them a sign of inflammation of the arteries, and this inflammation as the cause of the infarction. Later on Heschl§ looked on the coagulation of blood in the arteries supplying the part as the original occurrence, and considered the formation of the infarction as arising from this, representing the whole as a hæmorrhagic inflammation. This view of the disease as an inflammation, as a pneumonia, has also lately been again taken up. But it can hardly indicate in any sufficient way the nature of the disease. The whole matter took a different turn from the works of Virchow and Panum. Virchow‖ recognised the most of these decolorised adherent clots in the pulmonary artery, which had been previously viewed as the results of inflammation or stagnation of blood, to be really emboli. Panum¶ produced, by means of artificial emboli, affections in the lungs of animals which could hardly be distinguished from the

* Traité de l'auscultation médiate. Ed. iv. p. 119.

† Ueber den Lænnec'schen Lungeninfarktus, etc. Erl. 1850, p. 31.

‡ Prag. Vierteljahrschrift. Bd. ix. 1.

§ Prag. Vierteljahrschrift. Bd. liv. 21.

‖ Gesammelte Abhandlungen, p. 224.

¶ Virchow's Archiv, xxv. 433.

hæmorrhagic infarction. There seems to be little doubt that most of the infarctions arise from the embolic closure of the supplying arteries. The wedge-shaped piece of tissue with its base at the pleura which the artery supplies, is, after closure of the artery, the seat of a hyperæmia, the blood flowing backward from the veins and infiltrating the part. The tissue is loosened in its structure and infiltrated with blood, which coagulates and converts the part into a swollen granular reddish black mass, over which the pleura is generally coated with a layer of fibrine. Lænnec frequently oberved his pulmonary apoplexy in the middle of a lobe, and similar central ones which have more of a rounded form, have frequently occurred to me. Cohnheim's* doctrine, according to which it is only the plugging of (peripheral) end-arteries which leads to infarction, is liable apparently to frequent exceptions, under the influence of general or local disturbances of the circulation which may be unfavourable to the supply of blood by collateral vessels to the part undergoing infarction. Indeed the doctrine of the embolic nature of the infarction which we here advocate, leaves out of view a number of facts as undoubted exceptions, namely, the infarction without discoverable embolism, and the embolism without infarction. The former of these is met with in certain infective diseases, the latter under very various conditions, such as too rapid occurrence of death or a high degree of anæmia. So that beyond an embolus the lung-tissue may either remain unaltered or be in a state of simple œdema, or of red or grey hepatisation, or in a state of infarction, or of abscess and gangrene. In cyanotis, or at least in persons who are not anæmic, a simple non-septic embolus will produce a hæmorrhagic infarction with the same probability as it would in the brain, kidney, or spleen.

Just as, in order to the complete history of a child, you inquire as to the condition of its parents, as to the act of birth, and then as to the child itself, so you may divide the complete history of an infarction into (1) the *initiating thrombosis*, (2) *the act of embolism*, and (3) *the infarction itself*. You will not always be able to make out clearly at the bed-side the relation of the processes, but the more earnestly you strive to do so the more frequently will you succeed in gaining convincing evidence of cause and mode of action, origin and propagation of a morbid

* Untersuchungen über die embolischen Processe. Berlin, p. 72.

process, and from such evidence you will deduce diagnostic conclusions and retrospects, and obtain a basis for prophylactic measures.

Two years ago there lay in bed No. 84 a young man who was near the termination of a slight attack of typhoid fever and was already free from fever; suddenly there supervened pain in the calf and œdema of the foot. The conclusion at once suggested itself that we had here a coagulation in the veins of the calf, such as so frequently devolopes during convalescence. A small infarction was soon detected in the lower lobe of the left lung, but it did not produce any inconvenience. The patient was forbidden any violent exertion, especially standing and walking, because this would facilitate the breaking off of pieces of the thrombi. All our preaching was however in vain, he made a short walk in the ward while the physician's and nurse's backs were turned, but it was soon interrupted by the occurrence of a suffocative attack, by a faint. While he walked in the ward the coagula in his calf became loose, and passed by the only possible way to the branches of the pulmonary artery. After this obvious embolic act there occurred large infarction of the lower lobes, then pleuritic exudation on both sides, and we were glad after two full months to be able to dismiss the patient as cured; he was now obedient enough.

THE SEAT OF THE THROMBOSIS which gives off the embolus causing the infarction may of course be in any of the systemic veins but not in the portal branches; it may be in the principal stems or smaller veins of the lower limbs, or in only one of them, more frequently the left; it may be in the plexus around the uterus, or in the prostate, or in the veins of the kidneys, where thrombosis in infants has been so well described by O. Beckmann, and the diagnosis brought closer by O. Pollak; it may be in the sinuses of the brain or in the right side of the heart.* Among fifteen cases which I collected here, there were five times old clots in the right auricle. These were mostly cases of heart disease, to a smaller extent of emphysema. It is often difficult, even on post-mortem, to determine the original seat of the thrombus in a

[* In the newly-born, thrombi from the ductus arteriosus Botalli projecting into the pulmonary artery, may afford the material for emboli of the pulmonary artery. Compare Banchfuss, in Virchow's Archiv, Bd. xvii.—MS. note by Author.]

clearly demonstrable case of embolism of the pulmonary artery. At least we ought never to come to the conclusion that a clot from the *left* side of the heart or from a systemic *artery* has wandered over to the venous circulation, or that the infarction has arisen from the rupture of a pulmonary vessel, and the coagulation has afterwards arisen in the pulmonary artery without a thorough examination of all the systemic venous system. Secondary coagula are very readily deposited in front of and behind an embolus, but they are with care readily distinguishable from it.

*The symptoms of thrombosis of the veins* are most distinct when it has its seat in the extremities, where, especially in the inferior extremities, we frequently encounter it as thrombosis of marasmus, of dilatation, and even traumatic. Then again the left is more frequently involved on account of the less favourable course of the iliac vein on this side. The principal signs are pain in the part especially in the calf; hard venous cords which can be felt; beneath these, dilated veins, œdema, cyanosis. In the case of renal thrombosis hæmaturia, and in that of the cerebral sinuses, the objective signs which have been lately so much discussed, may serve as means of diagnosis. It is indeed frequently the supervention of infarction of the lungs which first gives a certain basis for the diagnosis. Of particular importance in favour of the supposition of such a venous thrombosis is the discovery of valvular disease of the heart such as to lead to considerable overfilling of the right heart and of the systemic veins. This is particularly the case with mitral and tricuspid disease. In these cases it is not always the diseased valves or the dilated portions of the cavities of the heart which furnish the emboli, but much more frequently, it is distant, highly dilated portions of the venous system. Bockdalek found among 59 cases of hæmorrhagic infarction (1.3 per cent. of the post-mortems) 38 produced by heart disease. Willigk* more recently found it mostly as an accompaniment of disease of the heart and blood-vessels (he met it in 2.4 per cent. of the bodies examined during five years). Wrany† among 17 cases found heart disease 12 times. When thrombi of the right side of the heart precede the infarction it

* Prag. Vierteljahrschrift. Bd. I. p. 33.

† Ibid., xxiv. p. 7.

is often possible to suspect the source from weakness, irregularity, and acceleration of the heart's action and from increase of the cardiac dulness. In this case also the rapid appearance of large infarctions, for which no other thrombotic source is obvious, may confirm this suspicion.

The statement that aortic stenosis is peculiarly apt to cause pulmonary hæmorrhage, is explained according to my experience by the frequency of thrombosis of the right side of the heart, and emboli arising there. The ultimate cause of the frequent occurrence of spontaneous coagulation of blood in heart disease is unknown, we can only guess that it is related to the diminution of the blood-pressure in the arteries, the increase of pressure in the viens, the retardation of the current, and the altered nutrition of the walls of the vessels. Among the *infective diseases*, typhoid fever and dysentery are those which especially lead to infarction. Thus among 250 post-mortems of the former, Hoffmann found it 15 times, and among 80 of the latter, A. Burkart found it 7 times. Also small-pox, cholera, puerperal fever, pyæmia, often present infarctions; further, a number of diseases accompanied with local suppuration, as caries of the temporal bone, boils, etc. Strongly marked œdema around such purulent collections may direct attention to the filling of the veins with embolic material. Eleven years ago I noticed in one of my students several furunculi and very œdematous swelling of the face. I saw him again several weeks after, when I was consulted as to the treatment. I found that during the existence of the furunculosis he had an attack of "pneumonia without fever," with shivering, pain in the side, abundant expectoration of blood; following this there had developed a pleuritic exudation which was also without fever, but accompanied with violent dyspnœa. Remembering the boils and œdema, the whole connection of the case became clear; namely, thrombosis of the veins of the face, embolism, hæmorrhagic infarction, pleurisy. Finally perfect recovery ensued.*

THE ACT OF EMBOLISM may occur almost or altogether without symptoms, where only very small and few coagula pass

* The clinical history of this and a similar case will be found in a dissertation by A. Heydenreich, Ueber einige Quellen von Embolie der Lungenarterie. Jena, 1867.

into a perfectly sound lung, or when there is already a strongly marked dyspnœa. Very large or massive emboli introduced simultaneously cause sudden death, or unconsciousness with convulsions ending in death. Between these extremes lie the symptoms which in very various degrees indicate the obstruction of a portion of the pulmonary vascular system, and generally precede the proper symptoms of infarction. The more extensive the closure of the pulmonary artery, the more certain is the *attack of unconsciousness*, which opens the scene. The sudden diminution of the supply of blood to the brain is its cause. Under certain circumstances convulsive movements are added, and these are also explained by the cerebral anæmia, just as they may occur along with embolism of several cerebral arteries. In cases of severe coma, the stools and urine may be passed unconsciously as in Panum's experimental embolisms. In other cases there is a very temporary unconsciousness, a weakness of the limbs, a loss of the special senses while the intellect is pretty well preserved or only weakened. One of my colleagues and best friends who suffered from such attacks, complained in one of the first of them, that he saw nothing but a grey surface; his hearing was retained. He recognised persons present only by their voice. From all I have seen, the more severe embolic attacks first attract attention by the disturbance of the cerebral functions. To this class of cases belong, among others, many of the faintings and sudden deaths during parturition and delivery, especially in placenta previa, after hæmorrhages, separations of the placenta, and formation of thrombi. The meaning of such a fainting fit may be more easily made out from accompanying and preceding circumstances. It is true that it may occur during perfect rest in bed, even in sleep. But it is very frequently during unaccustomed muscular exertions, resumed it may be for the first time, that the attack occurs, such as the first rising from bed, the first going out, sometimes the movement of the bowels. Frequently it is dropsical patients in whom incision of the swollen parts has been made, or cardiac cases to whom at the very last digitalis had been given,* frequently slight attacks of dyspnœa or bloody sputum have pre-existed.

* Compare the first two cases of H. Hopf, Zur Diagnose der hämorrhagischen Infarkte. Diss. Tübingen, 1865.

Shortly after such an attack, we find the patient very much altered, the agony of death in his features, which are sometimes pale, sometimes waxy white, or sallow, sometimes pale and leaden in hue, or with a marked increase of a previously existing cyanosis. A narrowing of the pulmonary artery suddenly arising makes the arteries empty and the veins overfilled. The skin of the extremities, often of the whole surface, is cold, and covered with a clammy sweat. The radial pulse may be absent, at any rate it is small, empty, and without tension. Added to the actual emptiness of the arteries there is the anæmia of the muscular tissue of the heart, which renders its contractions less vigorous. Two conditions at this time determine an acute extension of the cardiac dull percussion, namely, the accumulation of blood in the right side of the heart and the relaxation of the muscular tissue from the anæmia just mentioned. Fever and anæmia, as a general rule, cause dilatation of the heart and extension of the dull percussion. We may prove this very beautifully in short feverish states which end with a crisis, as pneumonia, erysipelas, angina, if we map out the cardiac dulness before and after the crisis. We see it also in anæmia and severe chlorosis, where the cardiac dulness diminishes with recovery.

Soon after the beginning of the attack there developes a very violent *dyspnœa*. If the fainting is absent, then this is the first symptom, otherwise it immediately follows that symptom. Orthopnœa is commonly met with. The patient breathes with shoulders, with alæ nasæ, lower jaw, and the vertebral column is straightened and bent at intervals. But even these spasmodic efforts, the forcible movement of the chest and contraction of the abdomen, do not diminish the agony of suffocation. Every word, every movement increases the dyspnœa. The respirations are unusually close together, and reach a high number in the minute. Such dyspnœa as that may indicate of itself obstruction of the pulmonary arteries, if the temperature is low, if the percussion and auscultation reveal nothing, and if we can exclude affections of the hæmaglobin itself.

While at the time such an attack gives the impression as compared with nervous dyspnœa of a very serious deep-seated affection of the respiration and circulation, so serious that it at once breaks down the strength of a powerful man, yet it is remarkable how completely the whole picture changes in one or

at most two days. The portion of the pulmonary circulation which is still unobstructed dilates as much as possible, the pulse again developes to its proper fulness, the heat of the skin and the colour of the face return, the dyspnœa diminishes to a quite bearable extent, and obtrudes itself only on rapid movement. This is the case, at least in the first embolic attack, and where the thoracic organs are not in too unfavourable a condition. If several emboli follow each other rapidly, as once a day for several days, then the recovery is ever slower and less perfect. The dyspnœa remains permanently, the arterial pulse is small and frequent, œdema occurs, or if it has been already present it increases rapidly. In patients who already suffer from a severe catarrh, the occurrence of embolism will readily call forth œdema of the lungs. Even this, however, may in a few days pass off, if the phenomena have not exceeded a certain degree.

Embolism of the pulmonary artery may produce *increase of temperature.* This has been experimentally determined by Bergmann, and confirmed by several others. One of my cases at Jena, which was published by Penzoldt,* proves the fact for human pathology. This was a case of valvular disease with thrombosis of the pubic plexus, and numerous emboli and infarctions in the lungs; the temperature in the rectum rose to 39°.5 (103° F.). Penzoldt also believes that he has shown from another case, that the rise in the temperature is not due to the dyspnoic muscular exertion, but to the embolism itself. I believe this all the more, because I think I have seen a similar rise in temperature produced by simple non-irritating emboli in other organs.

I have repeatedly shown the temperature-curve of a man who was affected with aortic stenosis and embolic aphasia; he presented for months a completely normal temperature, and only once showed a febrile condition for a period of six days. The morning temperature remained at or under 38° (100°.4), the evening temperature, began at 38.6 (101.5°), rose in the next three days to 39.3 (103°), and then went down again in two days to 38°. Along with the fever there occurred a stitch in the side in the left hypochondre, in two days enlargement of the spleen could be detected, and this slowly diminished afterwards; a friction-sound was also heard and felt in the splenic

* Ueber den hämorrhagischen Infarkt der lunge bei Herzkranken. Diss., Jena, 1872, and Deutscher Archiv f. Klin. Med., Bd. xii. p. 13.

region. In short, I can ascribe no other meaning to this attack of fever, than that it was a symptom of embolism of the splenic artery with resulting formation of an infarction.

I show you here thirteen temperature-curves of cardiac and emphysema cases in which the formation of infarctions was proved post-mortem, and in which during their whole residence in hospital the temperature was taken twice daily. Five of these remained free from fever, although in some the observations began long before the appearance of the infarction, and extensive embolism of the pulmonary arteries was presented in addition to the infarction. In some of the cases doubt may exist whether there may not have simultaneously existed, some other fever-producing cause; in two cases, for instance, the fever, which occurred late, seemed to belong to the breaking down of the infarction and not to the embolism. There remain only five curves in which the fever undoubtedly begins with the first appearance of the embolism or immediately follows it. On one occasion 38.3° (101°) is reached on the first day, and is not exceeded during several daily variations. In this case there is only once a rise to 38.5°. In the third curve 39° (102·2°) is reached, in the fourth 39·5° (103°), and in the last 40° (104°). You see from this how irregular in height and duration is the fever produced by pulmonary embolism. Still it is of importance to keep before us two facts. In the first place by repeated embolism a febrile state similar to that in pneumonia may be induced and may be continued for several days. In the second place the shivering, which occurs with or after an attack of fainting or dyspnœa, or may by itself mark the embolic act, is not produced by a reduction of the temperature, as you might suppose from the reduction of the respiratory surface, but by an increase of the temperature, just as in the ordinary shivering of fever. It is often merely a slight shivering that is complained of, at other times a severe shivering fit. Even a slight rise in the temperature may bring about the feeling of cold. According to what has preceded, there may occur as *signs of the embolic process*, sometimes singly, sometimes severally united: *fainting, dyspnœa, and shivering.*

*The embolus generally passes to the lower lobe*, directed thither by gravity and the stronger current; after the lower the middle and upper lobes may be affected. Where there is no special

circumstance directing it elsewhere the embolus generally passes *to the right*, following the stronger impulse of the blood-current. All the statistics prove this. In the cases we have had in this place we found infarction twelve times in the right and seven times in the left lower lobe, once in the middle lobe. Some of the circumstances which may determine embolism and infarction of the left side are these: (1) Weakening of the current in the right pulmonary artery by the previous occurrence of several or of large embolisms on the right side, or by thrombosis of the pulmonary veins (Penzoldt). As the first infarctions generally form in the right lower lobe, so the later ones are generally in the left. (2) Weakening of the current by contraction of the right lung, compression of pleuritic exudation, and similar causes. (3) Increased strength of the current in the left branches of the pulmonary artery produced by increased movement of the left side in right decubitus, may be at least supposed to be a cause of primary infarction of the left side.*

The earliest and truest of the SYMPTOMS OF THE HÆMORRHAGIC INFARCTION is *spitting of blood*. In my 15 cases it was observed 13 times. Once it occurred 8½, and a second time 24 hours after the shivering which marked the occurrence of embolism, in other cases it was two or three days after the attack of dyspnœa. At the same time you will understand that it is by no means possible always to determine perfectly the length of time intervening. The bloody condition of the expectoration may be only transitory, and may be in the form of a mere trace, or it may last a long time when the attack is very violent,—thus in two of our cases it lasted twenty-seven days. In infarction all forms of hæmoptisis which you can distinguish may occur, none is excluded. As a forerunner of the other forms we have often streaks and spots of blood in the muco-purulent mass; in very anæmic persons this form of hæmoptisis continues the only one. For the existence of expectoration of pure blood we have not only the evidence of Lænnec, who has seen almost incredible quantities of blood expectorated in cases of infarction, ten pounds in twenty-four hours, thirty pounds in fourteen days, but

[* I have since the publication of this lecture directly observed this: A patient had otorrhœa and acute brain symptoms, and embolism occurred while he was lying on his right side for the purpose of having an injection into the left ear. In the next few days the signs of infarction of the left lower lobe, and only in it, were distinguishable.—MS. note by Author.]

we have besides the observation by Dittrich* of moulded blood-clots in the broken up lung-tissue of the infarction. I have myself never met with pure bloody sputa in this disease. I consider this occurrence even more exceptional than the masses of mucus merely beset with bloody points. The sign which is commonly but falsely set down by many as pathognomonic of the hæmorrhagic infarction is the sputum intimately mixed with blood. It may be deceptively like the pneumonic sputum in colour, transparency, toughness, etc., but as a rule it is more bloody than this, and it contains no croupous coagula, certainly none of a light colour; it is rarely the only form present, but is mostly placed among muco-purulent sputa. This latter circumstance is explained by the small size of most infarctions (according to Lænnec one to four cubic inches, according to Rokitansky from a nut to a hen's egg in size), and by the existence of valvular disease, emphysema, and such diseases as readily lead to bronchitis. Bloody sputa may be yielded by one infarction for several weeks. They contain for more than a week unaltered blood-corpuscles. After two or three weeks there appear however in the expectoration granules and crystals of hæmatoidin, so that this form of bloody sputum is also met with in the course of the infarction. After a variable time, often about one and a half to two weeks, the bloody sputa, which have become scarcer, assume a brownish red, or dark red colour, similar to that of currant jelly. The expectoration of bloody sputa most frequently calls attention to the existence of the infarction; we learn often, for the first time, on putting direct questions, that a shivering or dyspnoic attack has preceded. At the same time this form of sputum does not prove the existence of an infarction. It is not found in every case and is not always a sign of this affection. Just as in pneumonia so here, the sputum is rich in an albuminoid substance which is precipitated by alcohol and readily re-dissolved by water, similar to what has been described, among others, as paralbumin.

Among the *local signs* there occurs in the first place, soon after the onset of the hæmoptisis, that crepitant râle which was already mentioned by Lænnec. It is distinguished by the size and fewness of the clicks, and by the seat being confined to a particular spot. The crepitating râle becomes by-and-by

* *loc. cit.*, pp. 4 and 5.

metallic, and the percussion-note becomes simultaneously dull at this point, which is mostly situated on the right side between the angle of the scapula, the vertebral column, and the diaphragm. According to the extent of the dulness we may have bronchial breathing. The dull percussion is generally easily detected on a careful examination of the lower lobe. Hopf, who is doubtful on this point, must have met with peculiarly unfavourable cases. With a size of one to four cubic inches, or of a nut to an egg, as we find it generally stated, and with a position which is preponderatingly peripheral, we should surely expect in the majority of cases to detect a distinct dulness. Where the whole or half a lobe is affected with a hæmorrhagic exudation, then the whole series of phenomena of lobar hepatisation will be met with and in the same degree. I have not often found, that contrary to expectation a large infarction had produced no dulness, much more frequently is it the reverse, that at a place where there was extensive dulness an unexpectedly small infarction has been found. The reasons are easy to perceive. Œdematous lung-tissue in the neighbourhood, increases the area of dull percussion. Then pleuritic exudation is very often associated with the infarction, and the dulness produced by this may be taken along with that of the infarction, at least when the latter has its seat near the lower edge of the lung. As the bloody expectoration may occur at a variable time after the embolic act, so the dulness on percussion, following as it does shortly after the bloody expectoration, makes itself evident sometimes one, sometimes several days after the dyspnœa or shivering fit. During several days it increases in extent and intensity and disappears very slowly, in fact there frequently remain traces of it for a long time. The dull note is often at the same time tympanitic. This arises partly on account of the surrounding œdematous tissue, partly because the note of a solidified portion of lung-tissue with open bronchi is by no means dull, as is generally supposed, but tympanitic, as has been determined by direct experiment. It is possible by a proper arrangement of the experiment to demonstrate in the tympanitic note of the hepatised lung after its removal from the body Wintrich's change of note. The bronchial breathing of an infarction comes and goes without proper fine crepitant râle. Unequal bubbling, coarse crepitant râles predominate. The

vocal resonance is increased in proportion as the bronchi are open, and no pleuritic exudation takes part in the dulness. In a third of the clinical cases, but much more frequently in persons who observe their own symptoms carefully, there is found a *stitch in the side*, which corresponds approximately to the seat of the infarction. It may also form a useful direction to the principal seat of the acoustic phenomena, and may frequently, along with the friction-sound heard in the same neighbourhood, be related to the inflammation of the pleura which touches the infarction.

Before we speak of the pleuritic exudation and the final results of the infarction, let us once more return to its beginning. The obstruction of a portion of the channels of the lesser circulation produces all the phenomena which are associated with reduction of the arterial pressure and the overfilling of the systemic veins. When secondary coagulation is added to any great extent to the original embolus, especially if this propagates itself backwards, then the disturbances of the circulation just mentioned remain or even increase; the quantity of urine becomes, after the embolism, distinctly diminished, dropsical swellings extend rapidly, the liver enlarges and becomes, on account of the stretching of its peritoneal coat, painful. Especially in repeated embolisms, and in states of weakness of the heart, does this increase of the disturbances of the circulation become prominent. Under these circumstances there often occurs a pretty severe icterus. The most obvious explanation of this is the passive hyperæmia of the liver. In fact we found the nutmeg-liver in several of the five cases of icterus, which occurred among our fifteen cases of infarction; during life biliary pigments were expressly determined in the urine. On the other hand in one of our cases we did not succeed, even by using chloroform, in determining the existence of biliary pigment in the urine. In cases outside the hospital I have often met with this. I would therefore hesitate to adopt for all cases the obvious explanation which the nutmeg-liver offers, namely, compression of the bile-ducts by the dilated veins. I would suggest the possible explanation in some cases, of the blood-pigment brought into a state of solution and absorption in the infarction.

All subpleural infarctions give rise to *pleurisy* just as pneu-

monia does. Only the occurrence of a fluid exudation is much more frequent than in pneumonia. In truth during hepatisation the mechanical relations are much less favourable to the formation of a fluid pleuritic exudation than with the infarction. In the latter the pain in the side is less commonly met with, and if present is less severe; on the other hand the pleuritic friction-sound is met with very often, and may last for a long time. Most of the pleuritic exudations of cardiac cases proceed from infarction. You should always ask, where there is pleurisy in cardiac cases, whether hæmoptisis was present at the outset. You may receive in this way at once insight into the connection of the phenomena and a standpoint for prognosis.

I have never seen a purulent pleuritic exudation proceed from a simple infarction, but very many sero-fibrinous, of which the greater number ran their course almost or altogether without fever. The pleura over the infarction first appears somewhat cloudy, dull, thickened, and at the circumference of the infarction slightly hyperæmic, then there occurs a fibrinous deposition in the form of a network, under which also little hæmorrhages show themselves. Afterwards we frequently find a thick fibrino-purulent membrane corresponding in its limits with the sinuate edge of the infarction whose surface is already sunk in and spoiled. The fluid exudation, which is of a yellow colour and of a flaky turbidity, or like whey, may reach such an extent as half to fill the pleural sac. Concerning this exudation we may remark that, besides the absence of fever, it has the peculiarity that it often produces dyspnœa disproportionate to its amount, and that it usually leads to dilatation of that side of the thorax. We have proved by very frequent and exact measurements that the expansion of the thorax from pleuritic exudations is properly a bilateral expansion, only it is much greater on the affected side. The property which exudations arising from infarction possess of expanding the thorax, however natural it may appear, deserves special mention. Pleuritic exudations which accompany cancerous disease of the lungs, are also accompanied by hæmoptisis, produce disproportionable dyspnœa, because here also a portion of the lung is impervious, but they lead to contraction instead of expansion of the half of the thorax. Pleuritic exudations arising from

simple infarction generally run a favourable course in spite of very violent symptoms, especially dyspnœa.

*The anatomical consequences of the infarction* are: (1) Dissolution of the rusty-brown portion of tissue, absorption of the blood in the alveoli with retention of pigment in the tissue. In this way does Rokitansky describe a perfect resolution of the lesion analogous to the resolution of a pneumonia. I have never been able distinctly to recognise any of the stages of this process, but I would not in the least throw any doubt on its existence. (2) Decolorisation and contraction into a yellowish white or grey cicatrix of small dimensions. This appears to me to be the most frequent and most favourable transformation of the simple infarction, and its existence may often be proved a long time after the conclusion of the process, being quite analogous to the course followed by other embolisms and infarctions, such as the so-called fibrinous wedge in the spleen or the yellow cicatrix in the brain. (3) Softening and liquefaction of the structure to a brownish red or greyish red odourless fluid, which discharges into the bronchi and leaves a cavity. This fluid is remarkable for the abundance of hæmatoidin crystals which it contains along with elastic fibres and large cells containing blood-corpuscles and blood-pigment. We may guess that this condition is present when, after the existence of bloody expectoration for weeks, there appear later on sputa of a striking greyish red colour. It is certainly not impossible that such cavities, the result of softening, may contract, may come to enclose a calcareous nodule composed of inspissated secretion, or may in some other way disappear, but with this simple breaking down, perforation of the pleura may result (in this way pneumothorax may be caused in heart disease) or gangrene with decomposition may supervene. When this form appears the prognosis of a favourable issue is very slight. (4) The hæmorrhagic infarction breaks down in the way of suppuration or gangrene. This consequence is generally determined by the constitution of the embolus, which carries with it some material which provokes suppuration and decomposition. With such infarction there is of course a purulent [illegible]osing pleuritic exudation. It is also probable that [illegible]hich provoke decomposition may be carried to [illegible] the inspired air in sufficient amount and active

enough. Gangrenous infarctions will be frequently recognised by the evil odour of the expectoration and breath, and purulent ones by the repeated rigors. Such processes have the worst possible prognostic significance. They are simply an intermediate stage in the process of putrid infection which seldom makes halt here, but extends further by way of the pulmonary veins. In addition they produce a series of local lesions; gangrene of the lung, pneumothorax, purulent pleuritic exudation, hæmorrhage from the lung.

*The diagnosis of the hæmorrhagic infarction* is, from these considerations, easy to make in most cases, especially in cardiac cases, and in cases of thrombosis of the crural veins or those of the calf.

Every session there are some cases in which the nature and seat of the infarction have been correctly determined. We have been able in many cases to save the patient from the shock of the hæmoptisis by timely warning of its approach. Yet errors may very readily be committed if we look on the form of the expectoration as pathognomonic. Very similar forms of expectoration occur under the following circumstances:

(1) Frequent *ecchymoses of the bronchial mucous membrane*, such as often occur in heart disease with endocarditis of the left side. You will therefore only count the infarction as certain, where you are able to determine the presence of dulness developing slowly during several days in one of the lower lobes.

(2) *Cancer of the lung*, whose symptoms have been very well described by Koehler and Walshe, furnishes, in the majority of cases, a few bloody sputa in the midst of many mucous ones. The former are intimately mixed, but very bloody, and somewhat of the colour of currant jelly. They resemble to a high degree the sputa which are discharged after an infarction has existed long. There is often cancer of other organs, especially of the thoracic glands, the thyroid, the lymphatic glands or the liver, and this may lead us to the proper diagnosis; in other cases pleuritic exudation with contraction of the side accompanies a unilateral cancer of the lung. In carcinomatous patients it will be very difficult to distinguish an infarction if it occurs from secondary cancer of the lungs, and it will, as a rule, only be possible when it is in the retrograde stage.

(3) *Echinococcus of the lungs* must also be considered on account of the sputum intimately mixed with blood which it furnishes. When we have a cyst which developed originally in the liver or peritoneum, growing up and breaking through, then, of course, the diagnosis is not difficult. Thus we had at the Poliklinik at Jena, a watchmaker from the neighbourhood affected with a large fluctuating tumour of the liver, who discharged at intervals for years echinococcus skins, along with a bloody sputum very similar to that of pneumonia.* In another case which I also met with in the same neighbourhood, which is pretty rich in echinococci, the diagnosis was for some weeks impossible. A young clergyman coughed up every day sputa intimately mixed with blood; he was perfectly free from fever, and apart from the inquietude from the hæmoptisis felt perfectly well. We found on the right side beneath the nipple and at a spot which might well enough correspond to the lower edge of the lung, a perfectly shallow note and diminished respiratory murmur. The matter remained problematic; we thought of infarction, till suddenly the sputa became more abundant and thinner, they contained several beautiful membranes and then lost their bloody character. Primary echinococci of the lung or those transplanted by embolism will only be diagnosed if they form a tumour at the chest-wall or if membranes are coughed up.

(4) The diagnosis of the infarction from *pneumonia* should never rest on an individual symptom, such as the appearance of the expectoration, the onset with an attack of dyspnœa or rigor, the presence or absence of high fever. Yet you may definitely hold the infarction as determined, if there obviously exists embolic material and an embolic attack, and when the diagnosis is confirmed by absence of fever, dulness in the lower lobe lasting for some weeks, and hæmoptisis. In cases where the subjective distress is very slight and hæmoptisis and a limited dulness in a lower lobe can be determined, we may very readily infer the existence of small infarctions. In this diagnosis, you must keep before you the whole history of the development of the infarction as we have gone over it here. If many individual signs are absent, all the others must agree, if our diagnosis is to be a sure one. Most difficult is the

* Described by M. Seidel in the Jenaischen Zeitschrift. Bd. 1, p. 293.

diagnosis where the infarction is accompanied by high fever, as in a case from the clinique in Jena, described by Penzoldt, and one which I met with here. We will not set down any model diagnostic rules, such as are calculated to expedite the economy of the intellectual process,—these vouchsafe to the weak a deceptive security, and never give satisfaction to the man who works with intelligent industry.

The *prevention* of diseases is doubtless a higher object than their cure. This applies also to the infarction. We possess indeed no means whereby to prevent the coagulability of the blood in the body, or to shut off thrombosed vessels from the circulation. This would be, if practicable, the most thorough means of prevention of the infarction. But how often is it proved to us by the existence of venous calculi, and tunnelled adherent thrombi, that clots may long lie innocuous in a vessel, and assume such relations to the vessel that all danger of their shifting their place goes bye. The following points seem to be of consequence in connection with the avoidance of the formation of thrombi which are prone to give off embolic material according to the length and looseness of the coagulum. The antiseptic treatment of wounds diminishes the danger of prolonged traumatic thrombi. The incisions which are made in the legs with such excellent results in cases of dropsy, not with a needle but according to Traube's rule, in the form of a long lancet-wound, should particularly be kept clean by frequent pencilling with chlorine water or chameleon (Condy's) solution. These very incisions are frequently the source of emboli in heart cases. For boils in loose parts of the skin which readily swell, the ice treatment of Hebra is particularly to be recommended, as it diminishes the spread of the inflammation and the swelling of the neighbourhood, and thus lessens the danger of thrombosis.

Here also is the place to say a word on the treatment of heart cases by digitalis. Cases where the valvular lesion is well compensated, and in which there is little palpitation, are often treated with digitalis as if this was a remedy for valvular disease. Shortly after the retardation of the pulse has been attained, the pulse becomes strikingly irregular and weak, then frequent and indefinite, all the troubles increase, the cardiac dulness becomes broader, and finally infarctions arise in the lungs which are referable to thrombosis in the heart. You should

septic inhalations. But on the whole this proposition is applicable: *The infarction, which is not infected either by the embolus or the inspired air, cures itself. The danger lies in the embolism. If the infarction forms, the danger is already past.* If the position of heart cases often gets essentially more serious after infarctions have arisen, the blame lies with the disturbance of the circulation from the closure of the pulmonary vessels, not with the infarction of the pulmonary tissue. To the already existing valvular affection and increasing the disturbance caused by it there is added a stenosis of the pulmonary artery.

I would not advise you to evacuate the pleuritic exudation arising from the infarction by aspiration. According to the above considerations, the softened infarction may in this way be led to perforate the pleural sac. The course of this fluid is in any case a favourable one. At first the disproportionate dyspnœa which it causes requires the abundant use of morphia, afterwards a tonic treatment is frequently sufficient, such as the use of iron and quinine. In extreme cases sudorific treatment is very well adapted to cause absorption of the fluid.

# THE GENERAL

# SURGICAL DIAGNOSIS OF TUMOURS.

BY

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GENTLEMEN,—I think there is no doubt that you as students look upon those pathological structures which we now commonly name tumours, in a quite different way in their relation to the other pathological conditions of the body, than did that generation of students to which I belonged. It has required much labour, and much abstraction from the prejudiced views transmitted to us, before we attained to the standpoint which we now have reached in relation to tumours. We must indeed acknowledge that in this department of pathology there remains much to be observed, and that the views of those who have specially studied the subject by no means correspond. Yet the study of tumours has now, so to say, stepped out of the period of storm and tempest. The literature of this subject was at one time enormous in quantity, and it clearly reflected the contrariety of views, the eagerness to find something certain and new, and to elucidate it in all directions which then prevailed; but this has now given place to quiet labour at the subject, and we have in the study of tumours reached, as it were, a stage of the mountain ascent, which we have to climb, where we can once more take rest and draw breath, and where we can enjoy a comprehensive view of what we have already attained.

It cannot well be otherwise than that the views as to the pathology and especially the genesis of tumours should still vary widely; there is nowhere in pathology as yet a completely finished chapter, we are everywhere in the midst of the work, or perhaps we are frequently only at the beginning of it.

But everything which has been accomplished in the way of pathological anatomy and clinical observation, has already yielded quite tangible results for actual practice. In my belief we have made very special progress in the *certainty of our diagnosis of tumours.*

It would be difficult to give you an idea of the uncertainty with which we used to proceed with the investigation of tumours, and the reason of this was, that it was always supposed that we had here to do with something foreign, something peculiar, in regard to which our usual diagnostic methods were insufficient. Thus we contented ourselves with a very general diagnosis, and to this day, wherever the views of pathological anatomy have not been incorporated into the actual flesh and blood of the practitioner, you will hear him always content himself with the simple diagnosis of "a tumour."

It has, therefore, been specially the work of pathological anatomy to clear up our views on this subject and to make tumours amenable to clinical diagnosis; it has taught us to use the same methods of physical investigation for new-formations as for other diseases. The services of Virchow in this direction can hardly be sufficiently valued.

I will confess that when I first took on myself the duties of a clinical teacher, but did not feel so very certain about tumours (although I was aware that I had efficiently gone over the subject from the anatomical side), I thought that a statement of C. O. Weber's to the effect that there seldom happened to him an error in the diagnosis of tumours, showed rather too much self-confidence.

I can only say, after a long period of independent experience, that Weber is right; it is possible, except in a diminishing minority of cases, to recognise the histological characters of tumours before their removal or anatomical investigation. And we may remark, that though the anatomical investigation of tumours is the one essential condition to our knowledge of them, yet the other factor, that of clinical observation, may possibly be undervalued. Clinical observers having been for a long time supplied with anatomical weapons, may now with unprejudiced eyes observe tumours in their whole course, and they should at last be in a position to bring the anatomical facts

into accordance with the clinical, and to draw conclusions from the clinical facts as to the structure of a new-formation.

The clinical observer has in studying the characteristics of a tumour a great advantage over the anatomist. He has before him the living material, in which there are many characteristics that are absent from the dead body and from preparations, and these when rightly comprehended, furnish very valuable diagnostic signs.

But we must not remain satisfied with the clinical diagnosis of tumours exclusively, and presume to give a classification of tumours according to purely clinical characters, as has been done from time to time in a mistaken way. A proper clinical classification must always correspond to the anatomical one, since at the last all clinical knowledge rests on anatomy and physiology. And so I think that we may gradually rid ourselves of the purely clinical nomenclature and through time give up certain current names, such as cancer, sarcoma, just as, at least in Germany, the clinical classification of new-formations has given place to the purely anatomical system, to which consummation I think I have contributed not a little.

Before we proceed to our proper subject, it is undoubtedly of importance for our understanding of it, to determine upon certain general conceptions and fundamental principles. In the first place we must properly define our *conception of a tumour*.

We are not as yet so far advanced as to be in a position to characterise the group of diseases which we denominate tumours by their etiological characters, and to place them thus in a similar position with the products of inflammation. The proper etiology of the true new-formations is still very little understood; we must therefore, in order to define this group of affections, abide by such other characters as definitively separate tumours from other new-formations. We must firmly hold to the idea of *new-formation*," and from this point of view, I have, in my definition of a tumour, excluded a large group which is usually included, namely, the cysts. There is no cyst which is not referable to some process which occurs otherwise in pathology, it may be the retention of secretion, the cutting off by inflammatory processes, it may be extravasation or exudation. If we exclude cysts, I think that the definition which I have previously given is the most correct,

namely, *increase of bulk by the new-formation of tissue, in which no physiological object is gained.*

Basing on this, I classify tumours as follows:—

I.—Cystic tumours.

II.—The proper new-formations.

A. New-formation on the type of the connective tissue.

1. Type of normal connective tissue.

*a.* Fibroma (and Myoma).

*b.* Lipoma.

*c.* Chondroma.

*d.* Osteoma.

2. Type of embryonal connective tissue.

*a.* Myxoma.

*b.* Sarcoma.

B. New-formations on the type of the epithelial tissue.

*a.* The cancers.

Addendum: Melanoma.

C. New-formations on the type of higher forms of tissue.

*a.* Papilloma.

*b.* Angioma.

*c.* Neuroma.

*d.* Adenoma.

I have thorough reason to be satisfied with this classification from the standpoint of the clinical surgeon, because the chief question which turns up in a given case is always this: *In what tissue has the tumour arisen?* This determines its pathological character and in consequence its diagnosis; and by the classification given above we are, in the case of the proper new-formations, at once asked to concern ourselves with the native soil of a neoplasm. And if we know whence a tumour has developed, then we have completed the greater part of the diagnosis.

From this it will have been already evident that I am a supporter of the theory of Thiersch and Waldeyer, the germinal laminæ theory, if you will. I am a supporter of this theory on clinical as well as on histological and embryological grounds. I acknowledge that the embryological as well as the histological side of this theory is very much the subject of dispute, yet I can with confidence state that clinical experience is very

much in its favour, and I must specially say that under the auspices of this theory we have made great advances in the certainty of diagnosis.

I still hold to this fundamental proposition, that tumours of the type of the connective-tissue series always remain within this type, just as epithelial tumours can only be produced from cells of their own type. In the majority of cases the new-formation also remains within the same type in its details, so that cartilaginous tumours commonly arise from cartilage, fatty tumours from fat-cells. In the epithelial type they usually preserve their character even more fixedly, so that cylinder epithelial cells produce cylinder epithelial cancer, whose secondary tumours are wont to preserve the same type; and glandular epithelium is always reproduced in cancers of this type, and even in the secondary tumours an arrangement is often presented quite analogous to that of the original gland. As the pigment-cells, whether connective tissue or epithelial cells, have the speciality that they during their whole life produce pigment, while the cells lying nearest to them may not, so these cells, when they form neoplasms, will here also produce pigment-forming cells, and thus give rise to melanotic tumours.

Thus it results, according to my view, that there are among tumours no transition forms from one type of tissue to another; mixed forms are possible, and we see examples of them in the melanoma which developes from a pigmented mole, in which the cells of the rete Malpighii, as well as those of the subcutaneous connective tissue, were pigment-forming cells; in that case melanomata may arise, which are composed of sarcomatous and cancer-cells.

We also understand how a papilloma which is covered with growing epithelium on a connective-tissue stem may become a cancer by the epithelium growing downwards, or how an adenoma by an excessive growth of the connective-tissue stroma may take on preponderatingly the characters of a fibroma or a sarcoma; but there is here no transition from a connective tissue to an epithelial type.

If this be the case, then you will easily understand how much our diagnosis gains in certainty when we definitely can say: this growth has developed in such and such a tissue, it must therefore follow such and such a type.

I speak here of course always of *primary tumours*, since in regard to tumours which are clinically called malignant, they are in their seat in no way bound to any one type of tissue whenever the formation of secondary nodules is concerned. To give here an example. You are aware that the bones have not the slightest relation in their genesis to the outer and inner germinal laminæ; you can therefore affirm concerning any primary tumour in bone, that it cannot have an epithelial character, that there is no such thing as primary cancer of bone. In this respect a thousandfold clinical experience corresponds with the theory. The cases of primary cancer of bone of which we know, are connected with bones in a superficial situation, especially the lower jaw, then the upper jaw. I saw one develope in the tibia. In this case the cancer grows from the mouth or the antrum of Highmore into the bone; or it may happen as in my case, where there was on the tibia an atheromatous cyst imperfectly extirpated, and its remains had grown into the bone and destroyed it. Also in the case of a fistulous opening in connection with necrosis or caries which has existed long, the epithelium may grow into the bone and form cancer there. This has now been several times recognised, and I have myself seen such cases.

When therefore experience teaches that primary cancer of bone only occurs under very special circumstances, we should, in a supposed case of well-marked primary cancer (say of the femur), have first to search whether there is not a really primary nodule elsewhere, as for instance in the thyroid gland. If a primary nodule were not found we should then set down the case as an exception, which would not certainly alter the rule, and for whose occurrence we should have to seek some other reason. I have gone into this subject especially because, as a matter of fact, you may be astonished to meet with such rare anomalies, as that in some locality where you were accustomed always to find a particular form of tumour, you meet with one which in this place may be designated altogether heterotopic. The conception of heterotopy of tissue is, as compared with former times, much limited, but on the other hand it occurs frequently within the same type of tissue, so that we find cartilage and mucous tissue in proper connective tissue.

This point introduces us to the consideration of a matter

of importance in regard to the study of tumours as a whole, and particularly their clinical diagnosis, I mean *the typical occurrence of certain kinds of tumours in particular parts of the body.*

Hüter remarks very properly in his general work on Surgery, that surgeons ought to write a regional treatise on tumours. It is one of the greatest gains which an extended clinical experience gives us, that we have learnt, from the topographical locality in which a tumour has developed, to recognise its character. This is a gain which might most certainly be determined by statistics, and has been so determined for some localities. The preponderating frequency of the occurrence of cancers in the lower lip and female breast, the special occurrence of myomata and fibromata in the uterus, are well known and very valuable aids to diagnosis. When we know facts like these there are only a few other diagnostic marks necessary in order to be able to come to a certain decision as to the nature of the tumour.

In the instances adduced the histological explanation of the occurrence presents no difficulty; the tumours cannot be considered heterotopic. But there are other localities in which certain tumours are found again and again, and yet they are to be looked upon as in the histological sense heterotopic; I am accustomed to call these in my clinique, *regional tumours*, because I apprehend that they must have a distinct relation to the region in which they occur.

I may here, in the first place, adduce an example, which is not taken from the group of tumours proper, but from that of cysts, I mean the dermoid cyst. These cysts are confined to certain quite determinate regions: the middle line of the head, neighbourhood of the eye, side of the neck, testicles, ovaries. If you find cysts in these places you may almost blindly predicate that they are dermoid, and you will always turn out right.

In the neighbourhood of the angle of the jaw, we find one form of tumour particularly often, the enchondroma; they are frequently subcutaneous, frequently related to the parotid, more rarely to the submaxillary gland. They are never directly connected with cartilage, they must therefore be designated heterotopic; their occurrence in these positions however is so

frequent, that we can very suitably use it as an aid to diagnosis.

We find further in the region of the ischial foramen beneath the buttocks, that myxomata occur with striking frequency, they readily grow into the pelvis and attach themselves to the sacrum, and sometimes have the form of the myxoma lipomatodes. Whence the mucous tissue here developes I cannot say, at any rate it is not from the subcutaneous fatty tissue.

A region particularly remarkable for the occurrence of tumours is the crural region. In this locality the myxoma lipomatodes is again not infrequent, interchanging certainly with the lipoma; but further, this is a classical seat of the parosteal osteo-sarcoma; most of the cases of this latter tumour which I have seen and which are recorded, have been observed in this region. Let me add that I have extirpated in the same region a large cysto-adenoma which was seated deeply and had no connection with the skin.

Near the axilla of women without any connection with the mamma, or at least easily isolable from it, we find tumours which we can always call adenomata, and from which cancers may afterwards develope.

This continually repeated occurrence of similar forms of tumours in certain localities furnishing us with an admirable aid to diagnosis, suggests to us the idea that we have not here mere accident, but something according to rule, although up to the present time the final causes of this occurrence are not yet fully determined.

For the case of the dermoid cyst the law is easiest to determine. They occur in those situations where during embryonic life clefts exist, into which the external germinal layer penetrates, and where in an irregular manner a penetration of this layer may occur; for it is now generally acknowledged that these dermoid cysts are not to be regarded as anything but penetrations inward of the external integument.

I entertain the belief that the heterotopic occurrence of enchondromata in the neighbourhood of the jaw is to be similarly regarded, that in the formation of the ear, and in the formation of the salivary glands an embryonic inclusion of cartilage occurs which afterwards developes to a tumour.

I am unable to give any similar explanation of the occurrence of myxo-lipomata of the buttocks, and of parosteal sarcomata

and other tumours in the crural region, I must refer you to the embryologists and histologists, who may perhaps, as occasion offers, give us some information on the point.

But I will say this much, that for a long time I have been unable to rid myself of the idea *that heterotopic growths for whose development all physiological and pathological irritation* (which play such a large part in the development of cancers) *may be excluded as causes, might develope from germs included at these points at an earlier period.* We know very well from the history of cryptorchismus, that parts which have not taken up their proper position are peculiarly prone to the formation of tumours. As cellular elements which are included in later life, we may here adduce the example of the cicatricial cancer, and the cancer of bone developing from an old fistula. We know that the development of such included "aberrant" tissues proceeds slowly, and is often subject to physiological laws; thus the dermoids mostly develope from the time of puberty onwards; the adenomata of the mamma situated near the axilla, which signify nothing but aberrant mammæ without ducts or nipple, also develope at puberty or at the time of the first pregnancy. We need not therefore wonder, that the enchondroma of the region of the salivary glands only takes on its development later, and that this is also the case with the tumours of the crural region, although these generally show themselves pretty early, mostly before the twentieth year, in nearly all cases before the thirtieth or thirty-second.

I am certain, Gentlemen, that this view expressed by me, that such heterotopic and regional tumours develope from early included tissue-germs, will be much disputed, as indeed the proofs of it are for the present difficult to furnish. It has been very interesting to me to read lately a case of congenital myoma of both kidneys, described by Prof. Cohnheim,* in which the author resorts to an embryonic inclusion to explain the occurrence, just as I have previously assumed those very complicated processes which occur in the formation and changes of position of the sexual glands, as a basis for the explanation of the teratoma of the testicles and ovaries. These matters have already been taken up experimentally by van Dooremaal † (Donders),

* Virchow's Archiv, Bd. 56, part 1.

† v. Gräfe's Archiv, f. Ophth. ix. 359-372.

and with decided results. It is in the way of experimentation that explanations of these relations must be sought for, and this path has already been entered upon by Dr. Zahn, from whom we expect soon to have some published results.*

To return to our proper subject, we may now say *that most tumours are to be judged according to the character of the soil they spring from, that heterotopic growths connect themselves specially with distinct localities*, so that in this way clinical experience gains many definite footholds for diagnosis.

Looking for further aids to diagnosis we may, in the next place, bring forward *the occurrence of tumours in the various periods of life.* The various germinal laminæ do not seem to conduct themselves in the same way in regard to their productivity of growths. According to experience the growths of the connective-tissue type occur especially in the first half of life, up till about the thirty-fifth year, the epithelial after this time. Exceptions to this rule are indeed rare in the case of the epithelial new-formations, but more frequent in those of the connective-tissue series. This rule is so certain, that, for example, in the case of tumours of the female breast we may confidently state, that after the age of thirty-five, tumours other than cancers are an almost unheard-of occurrence; we refer here of course to the time of the first development of a growth. Cancer of the lip may indeed be designated a disease of old age.

On the other hand we find sarcomata presenting themselves early, they attack young people. Osteo-sarcomata occur more frequently in youth, but they are also observed later. Fibromata extend over the whole life.

Many tumours in their development are connected with physiological phases of life; thus most of the non-heterotopic enchondromata, and the greater part of the osteomata are related to the period when the skeleton is most actively developing. The lipoma (in so far as it is not congenital or produced by definite local irritation) occurs most readily in the so-called best years of life, simultaneously with the general development

[* Since this sentence appeared, I observe that Virchow, in regard to certain enchondromata of bones, has also admitted the probability that they may have developed from remains of cartilage, included at an earlier period of development. Sitzungsbericht d. Berl. Acad. der Wissenschaften, Dec. 1875.—M.S. note by the Author.]

of the subcutaneous fat. Puberty brings into active development the dermoid cyst, and the congenital adenoma of the sexual glands. The angiomata belong to early youth, they are indeed in the majority of cases congenital.

We may imagine that for the development of a certain number of the tumours at certain periods of life, we have a plausible explanation in the physiological stages of development; but this is much more difficult for the two large groups of the connective tissue and epithelial growths. Thiersch is of opinion that in later life the connective-tissue substratum of the body, bearing the blood-vessels and nerves, undergoes a certain exhaustion, so that on the one hand it is no longer prone to over-production, and on the other it does not offer a sufficient impediment to the epithelium when any irritation attacks the latter and gives rise to proliferation, so that the epithelium is able to penetrate deeply. By this theory may be explained cases of abnormally early occurrence of cancer.

Next to the age of the patient, the *course*, especially *the rapidity of growth* and the *size* of the tumour, are of consequence.

Those tumours grow most quickly which are best nourished and whose cells possess the greatest vitality. These are the tumours of the connective-tissue series, and especially the cellular tumours of this class, the sarcoma and myxoma. The growth of the sarcoma is often nearly as rapid, especially in early life, as that of an acute abscess, with which it may then be confounded. Its supply of vessels is as rich as that of young granulations, with which indeed it shows many analogies; we may in fact view granulation tissue as the physiological prototype of sarcomatous tissue. In sarcomata the vessels run freely between the cells, there is nowhere within the cellular mass any check put on their sprouting, and so it happens that while the young connective-tissue cells are of themselves active enough, they become still more so through the formation of vessels which keeps pace with their formation. The sarcomata may thus grow quickly to very large dimensions before they show any tendency to degeneration, which latter is unavoidable, if they depart too far from their seat of origin.

The forms of sarcoma, which we chiefly refer to here, are the round- and spindle-cell sarcomata of the soft parts.

It is only the myxomata which grow with similar rapidity, especially the recurrent forms. Their cells, apparently less than those of cellular sarcomata, require a rich supply of blood by large vessels; these cells are most nearly related to simple protoplasm, and it is in fact often astonishing what a small vascular connection with their seat of origin large myxomatous tumours which have grown very quickly often show.

The other forms of growths of the connective-tissue series, which produce not only cells but definite forms of tissue, grow correspondingly slower: the fibroma, lipoma, enchondroma, osteoma. The osteo-sarcoma, if it produces bony tissue, does not grow very rapidly; but if it produces only cells, as is the case with several central forms, then the growth is rapid, just as in the cellular sarcoma of the soft parts.

In regard to size, all these tumours may attain considerable dimensions, the lipoma and osteo-sarcoma particularly, give rise to gigantic tumours.

The epithelial tissues of the body are essentially dependent for their nutrition on the connective tissue; therefore purely epithelial growths can only attain a limited size, and readily degenerate when they are too far removed from their matrix. Look at an atheromatous cyst; the nearer its epithelial layers lie to the centre, the more certainly are they involved in retrograde metamorphosis and form the well-known atheroma. An epithelial growth can only have stability if the growing epithelium extends down into the connective tissue, where of course it finds nourishment enough for itself, or if the irritation of the epithelium is extended to the connective tissue whose growth accompanies that of the epithelium, so that the latter is supported by a stroma of vascular connective tissue. Without this connective-tissue stroma no cancer can persist, and we thus see that its nutrition proceeds in the same way as that of normal epithelium, by penetrating septa of connective tissue—vascular papillæ.

In this way cancers may form large tumours if they grow through a whole organ, and displace or supplant its tissue, but when they grow free, their growth is limited, and they then ulcerate much sooner than sarcomata. The cancers are thus tumours of moderate size and they grow slowly, because for their growth the co-operation of two forms of tissue is required.

If the epithelium grows too strongly, it must degenerate, and fatty degeneration and ulceration occur; if the connective-tissue stroma grows too strongly, then the epithelium being compressed by the well-known process of cicatrisation in young connective tissue, undergoes degeneration (scirrhus).

You see thus that the rapidity of development of a tumour, and its ability to form more or less large growths, may very well be used for purposes of diagnosis; we have already referred to the tendency to degeneration and to ulceration, because they are related to the mode of growth. The purely ulcerous form is special to the cancers, and in this the influence of locality comes prominently forward, for the early occurrence of ulceration commonly follows from some external injury.

Gentlemen,—During the growth of a tumour it may place itself in various *relations to the tissue in its neighbourhood*, and in the investigation of a tumour it must be always your first endeavour to assure yourselves in the most thorough manner of the nature of this relation. You will then see, that it either pushes aside its neighbourhood, or that it penetrates into it, to a certain extent merging into it. These two typical varieties are of extreme importance for our knowledge of the clinical relations of tumours, for we thereby distinguish the forms which have of old been designated *benign* and *malignant*. The malignant character was ascribed by the older pathologists to a tumour, when it possessed the property of producing tumours similar to itself in other parts of the body than its original seat, that is to say of becoming constitutional, an expression which will be familiar to you especially in regard to syphilis; the older writers indeed inverted the order, and said that the primary tumour is already the expression of a constitutional disease. If we analyse these ideas now, according to our methods, we find that the benignity and malignancy of a tumour are essentially dependent on its relation to its neighbourhood. Those tumours which displace their neighbourhood, push it aside, are the benign ones; they may adhere to the neighbouring tissue, but this only occurs in the way of inflammation by connective-tissue adhesions; they may cause erosion of bone, but this happens by pressure in the same way as occurs with large aneurisms; they may cause atrophy of neighbouring

muscle, press on nerves, and so call forth painful feelings, but all this occurs in the way of a foreign body acting on the tissue, while the growth of the tumour is included in itself; the tumour remains a separate whole and remains so during its whole existence.

In other tumours, you are at a very early stage unable to recognise any distinct boundary from the neighbouring tissues; as soon as they are felt they have already the character of an infiltration; they may indeed grow outwards, but at the same time, penetrating deeply, they plant themselves in the place of the tissue, they substitute themselves for it, grow through it, or penetrating the tissue and constituting with it a single mass, they thus form tumours. These are the malignant tumours; for they do not confine themselves to one place, but form, as already remarked, secondary tumours, or at least they show the tendency to recur in places where it was thought that they had been completely removed by surgical operation.

To the tumours of this latter series, the malignant ones, the cellular tumours furnish the largest contingent. It is no longer so difficult as it used to be for us to form an idea of the processes by which these growths acquire the peculiar character of recurrence. No one any longer doubts the power of young connective-tissue cells to alter their position, and in this way a young sarcoma-cell, to take this as an example, may creep into the tissues, and gain by-and-by the lymphatic current, which carries it into the blood through the lymphatic glands; in the blood it may establish itself in the tissues and produce new tumour-cells, this occurring most frequently in the region of the lesser circulation. It may be caught in the lymphatic glands, but this occurs rarely in the case of the sarcoma-cells, they are not large enough to be compelled to make a stoppage here. Turning to the epithelial tumours we are able to represent the process as similar, for proof has now been furnished of the power of motion, of wandering, possessed by young cancer and epithelial cells. But the young cancer-cell is on an average considerably larger than the young connective-tissue cell, and when it reaches the lymphatic current it is generally caught in the nearest lymphatic glands, where it settles and forms secondary cancers, which may then form tertiary depôts.

In this difference in the properties of the cells of tumours,

by which they either pass through the lymphatic glands or make them the seat of secondary tumours, you have a criterion of the character of the original tumours, which in most cases will be of value.

It will not be proper for me to linger too long on the other possibilities of propagation of the primary tumours, I will only remind you that the penetration of a tumour into the tissues will lead to embolism of the substance of the tumour if veins have been penetrated. We do not now require to suppose an infective juice. The local recurrence of tumours may have its explanation in the fact, that, on account of the impossibility of recognising their boundaries during extirpation, they are not completely removed. In this way other tumours besides the cellular ones may recur, as diffuse lipomata, diffuse fibromata, adenomata, angiomata. Yet the greatest tendency to recur is presented by the cellular tumours, because the microscopic tumour which has been already begun outside the original growth, cannot be recognised either by the knife or the eye, and it is therefore left, and grows on.

As the points in the diagnosis which we have been discussing lead us at once to think of the prognosis, it may be as well here to enumerate the tumours according to a clinical scale of malignancy.

In the first rank stand the cellular sarcomata, with the exception of the giant-cell sarcoma, whose peculiarity in this respect is explained by its being composed of gigantic cells. This leaves as in the first rank of malignancy the round-cell sarcoma, to which belongs the myeloma (the myelogenous osteo-sarcoma), the spindle-cell sarcoma, and the melanoma. In the second rank stand the cancers, the osteo-sarcoma, the myxoma, the myxoma lipomatodes, the enchondroma myxomatodes.

These are the properly malignant tumours whose cellular nature determines this character. In addition to them there are certain tumours which present a strong tendency to local recurrence, the giant-cell sarcoma, the adenoid sarcoma, and the fibro-sarcoma, and then also some fibromata and adeno-fibromata.

The rest of the tumours are benign.

We have in the previous remarks attended to a number of

general points which will aid us in the differential diagnosis of tumours, and which were specially taken from their physiological relations. There still remain for our consideration the *physical peculiarities* and the points in the *history of their growth.*

Turning to the peculiarities which the eye is able to recognise, we may in the first place refer to *the colour* of a tumour. It is only possible to observe this in cutaneous or subcutaneous tumours, or such as lie in cavities which we can reach, or in the eye; but in these situations pigmentation of a new-formation is easily distinguished, and so the melanoma is easy to recognise. At most it may be confused with a vascular tumour covered with healthy skin, especially the cavernous angioma, but it has not many other characters in common with this form. The simple angioma is quickly recognisable, because its new-formed vessels are distinctly delineated. In other cases again the inspection of the tumour may, by changes in the colour of the skin, enable us to recognise the relation of the skin to the tumour beneath; the skin may be shining, which indicates that it is stretched, it is often slightly red, which may be related to commencing ulceration, it may be free from pigment, as occurs in cicatricial contraction.

The eye teaches us further the *size* of a tumour, the significance of which we have already discussed, but above all it shows us its *form* and the condition of the surface. The form of a tumour is in general too much dependent on accident to be of much use for diagnosis. Tumours which have grown in cavities very generally take on a pedunculated form; this applies especially to the tumours of mucous membranes, which were formerly comprehended in their entirety under the designation of polypi. This similarity of form does not in any way permit of a conclusion as to the quality of the tumour; polypoid tumours of the rectum may be adenomata as well as cancers, and the same applies to those of the nasal cavities. Hardly any tumour presents under all circumstances the same form, only the papilloma appears to possess something characteristic in its fissured villous appearance, but even in this respect certain forms of cancer concur.

It is otherwise with *the condition of the surface.* Many tumours are smooth, as in most cases the cysts, and approximate

to the globular form; others are lobulated as the lipomata and adenomata; others are largely granulated and knobbed, as many osteomata and fibromata, and others are finely granular, as the enchondroma. The malignant tumours are in this respect perhaps the least to be reckoned on; on account of their mode of growth they long cling to the form of the part in which they have their seat, so that the part appears only to be enlarged, they may—especially the sarcomas—long preserve a smooth surface, but they may then become knobbed and granular, and if anything is characteristic it is the rapid occurrence of isolated prominences, which often take on a distinctly globular form, and then rapidly adhere to the skin and proceed to ulceration.

If we see that an ulcer has formed on a tumour, our first thought will be that the skin has become involved in the disease, that we have to do with a malignant tumour; and if the skin in the neighbourhood of the ulcer is discoloured and infiltrated and the ulcer elevated, then our diagnosis is pretty secure. For, other tumours ulcerate the skin essentially by pressure, it is not infiltrated, it is rather thinned and then destroyed, its edges are undermined and inside them the substance of the tumour is visible, and the latter may itself be ulcerated. This is what happens in the case of the osteoma, enchondroma, and lipoma.

We have particularly to advert to *transparency* of tumours, which has hitherto been recognised almost alone in tumours with fluid contents. This peculiarity can only be investigated when the tumour is to some extent isolated from its surroundings, most conveniently in tumours of the testis. All fluids which are little coloured present transparency if they are not inclosed in too thick a sac; serum, mucus, colloid, even fluids containing cholestearin and spermatozoa allow the light to be transmitted; let me add that chronic abscesses also, in which the corpuscular elements of the pus have settled at the bottom, allow of the transmission of light. Of the solid tumours, a homogeneous sarcomatous tissue, and the myxomatous tissue, are transparent; the same property is possessed also by some lipomas and simple hypertrophies of lymphatic glands.* I have only of late directed my attention afresh to the transparency of solid tumours, incited thereto by a diagnostic error; but I think that it is necessary to work out this chapter of surgical diagnosis

* See the Centralblatt f. Chirurgie, 1875, No. 47.

better. It will not be difficult to devise proper methods for a more intense illumination of the tumours to be examined. To begin with, the most essential result is this, that I must warn you against taking all transparent tumours for tumours with fluid contents.

Even if you are able, Gentlemen, to establish the diagnosis of a tumour in many cases with the eye alone, yet you will rarely neglect the *investigation by touch*. It is seldom necessary to test the degree of tenderness by strong pressure on the new-formation, this is given by investigations directed to other objects. One of the most important of these objects is to determine the *consistence* of a tumour. We effect this best by fixing the tumour with one hand as well as possible; or getting an assistant to fix it, if we need to examine with both hands. The hardest tumours are the bony ones and the calcified, therefore the osteoma and the calcified chondroma and fibroma, then the periosteal and central osteo-sarcoma, in which however the bony shell is often yielding. Then come the connective-tissue fibrous tumour, some fibromas, and scirrhous cancer. Of elastic hardness are the enchondromas, most primary carcinomas before softening has occurred in them, and again certain fibromas, as well as the giant-cell sarcoma and fibro-sarcoma. Of elastic softness are the soft fibromas, adenomas, lipomas, cysts with thick walls. We must designate as soft, the most of the sarcomas, the myxomas, the soft cancers, and many secondary tumours.

*Fluctuation* indicates that the tumour has fluid contents, therefore especially cysts, cysto-adenomas, and cysto-sarcomas. But the soft cellular tumours with abundant intercellular substance, the tumours previously called medullary tumours, present also a kind of fluctuation, which we may denominate *pseudo-fluctuation*, but which we are by no means always in a position to distinguish from actual fluctuation. This is easily understood; for the feeling of fluctuation depends, for the most part, on a molecular shifting of the contents of a tumour effected by the pressure of one finger through the mass of the contents against the other, and this happens as well in soft cellular and juicy sarcomas and also myxomas as in small tumours with fluid contents. In order to allow of the feeling of a wave-impact in a tumour, it must contain a large quantity of fluid and have a yielding covering. If a tumour has a tightly-stretched capsule,

or if it lies under a tightly-stretched fascia, then it is often difficult to determine whether we have a real fluctuation or a pseudo-fluctuation. It is similar with very movable tumours which, when examined deeply with the finger, move off. You will easily see that it is often difficult to determine the consistence of a deeply-seated tumour, and in that case it is allowable to avail ourselves of the acupuncture needle to throw light on it, or, in order to prove if the contents are fluid, we may make a trial puncture.

For this kind of investigation the exercise of the sense of touch in the finger-point is required, and this sense can be brought to great perfection for the recognition of fine differences. And you will do well to let this education be expended chiefly on the index-finger, because it is for all kinds of investigation fittest, both from anatomical situation and practice in ordinary life.

In the tactile investigation of a tumour you will direct your attention at once to *pulsation*, by which, in the first place, aneurisms will be distinguished from tumours. There are indeed pulsating tumours, and others in which pulsation is communicated from neighbouring vessels. The distinction is frequently very difficult. As to the pulsating tumours, in the first place these are either sarcomas of the soft parts or of the bone; we find in these growths no vessels of large calibre, but on the other hand an enormous vascularisation with small arterial and especially capillary vessels, which usually run between the soft cellular masses of the tumour without a proper connective-tissue support. The walls of the vessels in many such pulsating central tumours of bone, are often thickened by cellular growth in a similar way to what we see not infrequently in simple angiomas, so that we might almost set them down as central angiomas of bone, if their course did not correspond to that of the sarcoma. The reason that in these tumours which are thoroughly interpenetrated by capillaries, pulsation is to be felt, and even a kind of bruit is audible on placing down the stethoscope, may be that the vessels run between the yielding soft cells, directly in contact with the fluid or protoplasmic intercellular substance, and so the impulse coming from the heart meets with no obstacle, as it does in the formed connective tissue which accompanies the vessels in the cancers. The condition is similar to that in pul-

sation of the brain, or the pulsation of granulations which, as surgeons, we see so often when fluid is met with on a granulating surface. As these tumours are constructed like sponges, they may, like sponges, be diminished by pressure, and this peculiarity, along with the pulsation, makes it, under certain circumstances, very difficult to distinguish such tumours from aneurisms. The seat or mode of origin and course will, in most cases, lead us to the proper diagnosis, and we may say, on the whole, that when we have to do with tumours of bone, aneurisms may be excluded, as the medullary cavity of bones cannot well be the seat of a spontaneous aneurism, but such tumours developing here must be designated central osteo-sarcomata. But still there are rare cases where, especially if injury has preceded, the distinction between a pulsating sarcoma and an aneurism is impossible.

Pulsation will be communicated to tumours lying over large arteries, especially when they exist under a certain tension acting from without. In this case there is no aneurismal bruit, and as the pulsation is properly a locomotion of the tumour, the diagnosis is, for the most part, easily made out, especially when the tumour can be raised from the artery or shifted aside from it. The case is here also more difficult, if on deep pressure such a tumour is able to move aside into a cavity or otherwise, and thus presents the appearance as if its contents could be emptied.

The property of emptying in whole or part their contents on continued pressure, is possessed by the angiomas as well as the highly vascularised sarcomas, and the former indeed possess it in a much higher degree. These are especially the cavernous angiomas, to which may be added the lymphangioma cavernosum, a rare form of tumour, frequently congenital but sometimes acquired. For the cavernous tumours we have a characteristic sign in the mode in which they fill up after being emptied; this does not happen at a bound, as in the sarcomas and aneurisms, but slowly, swelling up like a sponge which is laid in water after being squeezed. Confusion in this respect can only arise when a tumour can be pushed into the depth of a cavity or between yielding tissues, and again comes forward when the pressure is removed, or tumours with fluid contents which can be emptied into a cavity; in this respect we have

mostly to do with abscesses, especially cold ones, whose recognition will be possible from other characteristic signs.

In handling tumours we sometimes feel *crepitation* in them; this presents itself as a softish creaking, so-called parchment-crepitation, if a thin bony shell is bent in. It occurs in some central osteo-sarcomas which only possess a thin bony capsule, in chondromas of similar construction, in cysts of bone, and in other tumours arising in the centres or cavities of bones where the bony capsule is distended to a thin plate. A dense hard crepitation is presented by tumours which have a bony or calcareous skeleton which is moved or broken by pressure, or we may find it in cysts which contain concretions.

It is of course very important in the tactile investigation to direct your attention to *the relations of the tumours to their surroundings*. I will not go back on what has been already said as to the relation of the benign and malignant tumours to their seat of origin, I will only refer to a matter of practical interest which we have to observe in this investigation; and this, apart from the prognostic importance already adverted to, is the bearing the question has on eventual operative interference. If the tumour is adherent to the skin, we must not preserve the latter, if it is adherent to the underlying parts, we must be determined to include these in the circuit of our operation. In order to this we must accurately test the mobility of the tumour and the possibility of raising it, which is by no means easy with large deeply-seated growths. We may be deceived in some cases by the shifting of the mass of the tumour itself, or of lobules of the tumour on one another; or we may overlook the pedicle of the tumour reaching deeply; or finally we may move some important part along with the tumour; but this, on the other hand, will—as in the case of the larynx and trachea—generally show itself by the occurrence of disturbance of function. Tumours are least mobile and able to be raised when they are adherent to bones or fasciæ; adhesion to muscle commonly allows a certain degree of mobility, as the muscles themselves are movable. Tumours seated in the soft parts are of course most mobile when they have a capsule and the vascular connections are small; long thin pedicles do not hinder movement.

Before everything else the relation to the vessels and nerves

must be made out. If the operator has not beforehand set himself right in this matter, there may be the most disagreeable surprises in store for him in the enucleation of the tumour. Peripheral œdema points to pressure on or adhesion to veins, and these adhesions lead to the most critical results, if for instance you cut into the external or the internal or common jugular in the neck; for this reason we must always proceed in this region with greater care. It is known that, especially in the case of secondary tumours, and among these particularly the melanomas, the tumour may proceed from the wall of the vein; in the operations on such tumours particular care must be directed to their relations to the veins.

As it is not always easy in the case of large tumours to determine their relation to the larger arterial stems; it is advisable in all such cases to begin the operation by exposing the stem going to the tumour, in order to be able to ligature it immediately if need be.

The relation to the nerves has only to be taken into account in certain cases from the functional disturbances. *Painfulness* in a tumour has been commonly used as a diagnostic sign of cancer; and we find as a matter of fact that spontaneous pains very often occur in certain carcinomata. We know that this is the case in the majority of cancers of the female breast and of the uterus, while cancers of the lip, for example, seldom present this phenomenon. Besides these, the tumours of the nerve-sheath, and the proper neuromas, such as those which form in amputation-stumps, are known to be spontaneously painful; I have also met once with a lipoma which was similarly painful.* All tumours may of course be painful which press on a nerve at a place where it cannot turn aside. Further, a tumour may become painful when inflammatory processes are set up. Again, cancers and nerve-tumours are specially painful on pressure, while sarcomas manifest on the whole a comparative insensibility.

When functional lesions occur in the region of a nerve which is related to a tumour, the nerve is generally involved in the tumour, or at least much compressed; but on the whole, paralysis, especially motor, depending on the pressure of a tumour

* Correspondenzblatt f. schweizer. Aerzte, 1872, No. 3.

on peripheral nerves, is rather rare; sensory lesions, mostly slight in degree, are somewhat oftener observed. We may here also remind you of the paralytic lesions of the vagus which occur in goitre and in cancer of the thyroid.

You must not forget to establish the relation of tumours to *neighbouring cavities*. In tumours in the joints this is easy; it is more difficult, and errors are more likely, in tumours of the abdominal wall, of the pelvis, of the rectum, also in tumours of the bony wall of the thorax. Such errors are serious in tumours of the skull, especially cysts which may be confused with hernia cerebri; also in cancer of the upper jaw, of the orbit and nasal cavities, which often destroy the base of the skull and expose the brain.

We pass here beyond the bounds of tactile investigation, and some of the relations named can only be recognised by the occurrence of certain functional lesions, and from the statement of subjective impressions by the patient.

But on the other hand, the bounds of tactile investigation must now be further extended, and you must not always content yourselves with handling the tumour from without; no, you must when necessary, examine them from the apertures of the body, and the method devised by Prof. Simon of investigating the abdominal cavity from the rectum is of great consequence for the examination and diagnosis of tumours.

We shall finally add to the tactile investigation, catheterisation in the wider sense, by means of which we can determine the relation of tumours to the bladder and urethra.

To this department belongs also *acupuncture*, the investigation of the tumour by a more or less elongated needle. We will not generally use this method to determine the consistency of a tumour, we shall avail ourselves of it chiefly to find out the boundaries on the side which is beyond the reach of the finger, and in that case it may yield us valuable results; we may especially determine the limits of the destruction of a bone by a tumour pressing against it. You must however exercise care in using acupuncture, when you are exploring in the direction of cavities, especially towards the brain, or when there is a possibility of wounding large vessels.

It is hardly necessary again to remind you that all the

lymphatic glands in the neighbourhood of tumours must be thoroughly investigated.

*Auscultation and percussion* are in many cases of value; as to the former, I refer you to what was said about pulsation of tumours; as to the latter, we may investigate the cavities of the body where this can help us to determine the boundaries of a tumour; then the diagnostic discrimination between hernias and tumours is also based on the use of percussion.*

I will also remind you that the ophthalmoscope and the laryngoscope are indispensable in the investigation of the parts concerned. And we have now gone over all the means of investigation of tumours which are afforded by our senses and physical methods to hand.

A further method, nearly related to acupuncture, yet in a certain sense an operative proceeding, is that called by some authors Akidopeirastic. We understand by this the small operation by means of which we take out from the depth of a tumour a piece for microscopic investigation. This is done with a small instrument like a trocar, of which the best known is the harpoon of Middeldorpf. However rational this proceeding may be, it has come very little into practical use. The reason is that, in the first place, we can in ulcerated tumours more easily get a piece by the knife or scissors if it is at all necessary; further, the method is, of course, not applicable to hard tumours and those with hard shells; and lastly, we are unable for the most part to make any certain microscopic diagnosis from the scrap brought up from the deep parts, because the harpoon generally brings to light only tissues which are not characteristic, connective-tissue fibres, broken-down cells. The microscopic diagnosis often presents difficulties when we make sections of extirpated tumours, how uncertain then is the diagnosis from a scrap of the tissue of a tumour got by accident. I think therefore that you do not need to provide yourselves with a special instrument for this purpose.

[* I might add to this, that for the diagnosis of beginning central tumours of bone the *percussion of the bones*, contrived by me in order to recognise changes by the height of the note which the bone gives on being percussed, may also attain some importance. See Ueber die Percussion der Knochen, Centralblatt für Chirurgie, 1876, No. 43.—MS. note by Author.]

Allow me, in conclusion, to bring before you once more the course which I advise you to follow in the diagnosis of tumours, and to add a few words further.

You will, after you have convinced yourselves of the presence of a tumour, and while looking at it, direct your thoughts to the tumours which specially occur in this locality; you will next undertake an ocular inspection, and then pass to manipulation, testing the transparency, consistency, etc. If you have used all the means of physical diagnosis you will inquire into the subjective symptoms of the patient, and pass to the history of the case. To this latter belongs in the first place *heredity*, which deserves a brief discussion, though our knowledge of it is not very far advanced; we know only that inheritance is observed through several generations, particularly in the case of cancers, and every experienced surgeon will know in his practice at least some cases where parents and children suffer from cancer; we know nothing of the inheritance of the sarcomas; for the rest the chondromas, osteomas, and neuromas have been most frequently observed to be inherited. We shall not be able therefore to receive much aid to our diagnosis from heredity. The age of the patient is, as you know, of importance, and so also is the course and the mode of development. You will often in these inquiries be met with the statement on the part of the patient that the origin of the tumour was due to some injury or other. I think certainly that we have every reason to inquire into the local irritations which give rise to tumours. They belong to the most essential etiological phenomena in the development of tumours. They cannot however be used by us at present for purposes of diagnosis, this would be the case if distinct irritation produced distinct tumours, but this is by no means the case. Single irritations, as a blow or fall, as well as continued irritations, such as pressure, friction and irritation from secretions, are given as causes for the most benign and malignant tumours. The tissue which is irritated will, according to a wider general disposition, produce that form of tumour which, according to its nature, it is able to bring forth.

I think, Gentlemen, that I have as perfectly as possible brought before you the characteristic features of a general dia-

gnosis of tumours from the surgical point of view. I am well aware that I might perhaps have spoken of the recognition of secondary tumours in the internal organs, but your knowledge of the diagnosis of the diseases of the internal organs will come in here; this matter is of importance, because if the presence of such secondary tumours is established, then, with few exceptions, all thought of surgical interference must be given up.

The diagnosis of a tumour, resting on safe grounds, will lighten your way to the resolve to undertake operations which are often dangerous to life, and it will also make less difficult the determination, which is sad to a surgeon, to abstain from interfering.

# RESPIRATORY PARALYSES.

BY

FRANK REIGEL.

GENTLEMEN,—As you are aware, two essentially different offices are allotted to the larynx, on the one hand it contains the organs of voice and speech, and on the other it has to provide a perfectly free and patent canal for the entrance and exit of atmospheric air. For these two functions special muscles are provided.

Corresponding to the numerous modifications and modulations of the voice we find that the muscular apparatus provided for its formation within the larynx is relatively a complicated one; whilst that provided for the free passage of air in and out is a very simple one, only one pair of muscles being employed—the posterior crico-arytænoid. So that we have the numerous vocalising muscles opposed to the one pair of respiratory.

As we might expect, paralysis of the muscles of vocalisation varies very much according as one or more or all of the muscles are affected. With the discovery of the laryngoscope the old malady comprehended under the term paralysis of the vocal cords resolved itself into a series of distinct diseases. Nevertheless they all possess one common character—loss of voice (Stimmstörung).

Owing to the simplicity of the respiratory muscles of the larynx *their* paralysis is, on the contrary, always marked by the same symptom, viz. changed and impeded respiration.

Ever since the laryngoscope taught us the manifold nature of paralyses of the vocal cords endeavours have been made to divide them into different groups. According as they have been looked at from this or that point of view so the grouping has differed. In accordance with the two before-mentioned

functions of the larynx, on the one hand as an organ of voice, and on the other as an organ of respiration, it would appear to be most serviceable to divide the paralyses of the laryngeal muscles into two main groups—*phonetic* and *respiratory*.

*Phonetic* paralyses will then appear to be characterised by one peculiarity, more or less loss of voice without any impediment of respiration. According as the phonetic paralysis varies in intensity and extension from a simple want of tone to complete paralysis of all the laryngeal muscles of vocalisation so varies the loss of voice from the mildest form of hoarseness to complete aphonia. In a similar way the laryngoscopic appearances will vary according to the number of muscles attacked and the intensity of the paralysis.

It is not so with the second group—the *Respiratory* Paralyses. Owing to the fact that there is only one pair of muscles employed in the respiratory function of the larynx, it follows that these paralyses, together with the laryngoscopic appearances, will all have the same characters, the intensity of the paralysis being the only factor which can cause any change in the clinical symptoms. Now, seeing that the only action of the crico-arytænoidei postici muscles is to widen the aperture of the glottis, so that air may pass in and out quite freely, it follows that paralysis of this pair of muscles will be succeeded by difficulty of respiration, and that the voice on the contrary will be unaffected.

This would appear to be the most appropriate meaning to give to the terms phonetic and respiratory paralyses, though Turk has made use of the same terms in a totally different sense. Turk, for instance, calls it phonetic paralysis when there is an abnormally wide aperture of the glottis and consequently aphonia or hoarseness, when an endeavour is made to speak. And also when, during forced expiration, as in coughing, hawking, and straining, the cough is sharp and full.

The same authority applies the term Respiratory Paralysis to those cases in which there is a loss of voice not only in speaking but also in the respiratory functions. We have therefore in this latter form of paralysis, along with a defective phonation an improper closure of the glottis in coughing, etc., the closure of the glottis not taking place under any circumstances.

Owing to the two above-mentioned main functions of the

larynx and the consequent liability to their derangement, we prefer to divide these paralyses into two chief groups, and shall call those *phonetic* only in which the muscles administering to the voice are paralysed; whilst on the contrary we shall call those paralyses *respiratory* in which the muscles administering to the function of respiration alone are paralysed.

I need scarcely mention that we may get simultaneous paralysis of both these—to a certain extent—antagonistic groups of muscles, and consequently a want of power in both the foregoing directions. This third group we purpose calling *Mixed Paralyses.*

Phonetic paralysis is the most frequent of these three forms of disease, though the mixed is by no means uncommon; respiratory paralysis is however very rare. There are extremely few well authenticated cases of this latter form to be found recorded. Nevertheless I do not think it occurs with such extreme rarity as the number of cases recorded would lead one to suppose, for in one year I have come across five cases of this kind.

With your permission we will to-day discuss a little more fully the symptoms, etiology, therapeutics, etc., of this most important and interesting form of disease.

A little consideration tells us at once that the symptoms which this disease will present to us are of necessity so simple that we may anticipate their nature.

We must however bear in mind the true action of the crico-arytænoidei postici.

As I have already told you, there are in the larynx two groups of muscles which are to a certain extent antagonistic to one another; on the one hand there are the muscles which tighten and bring together the vocal cords, and on the other there are the muscles which separate the vocal cords. Now whilst the muscles which tighten and bring together the vocal cords have only an intermittent action in the production of the voice, the muscles which widen the glottis, on the contrary, are always in action, so that a free passage for the atmospheric air may be kept up. It is easy to decide, both by pathological observations and experiment, whether the usual condition of the glottis in quiet breathing is the result of a passive equilibrium between the muscles which separate and the

muscles which bring together the vocal cords, or whether it is due simply to the action of the muscles which separate the cords.

If the usual condition of the vocal cords in quiet breathing represents a state of passive equilibrium in all the laryngeal muscles, we ought to find the vocal cords in the same position when the constrictors and dilators of the glottis are both completely paralysed. This latter state of things arises when the recurrent nerve, which, as you know, supplies nearly all the muscles of the larynx, is paralysed. The same thing occurs when the recurrent nerve is experimentally divided. In all these cases however, whether the conductibility of the nerve be destroyed by pressure of a tumour, an aortic aneurism, a mediastinal tumour, a wound, or whatever cause it may be, the position of the vocal cords differs essentially from that which obtains in calm respiration. A special name has quite justifiably been given to this particular position of the cords, and, because the same position is found in the corpse, the term "*cadaverous position of the vocal cords*" has been applied to it. This cadaverous position of the vocal cords by no means corresponds to that observed during ordinary respiration; in the cadaverous condition of the cords they are found to be much nearer the median line. This position, which is to a certain extent a physical one, is the result of perfect rest on the part of the antagonistic muscles.

It follows therefore that that position of the vocal cords which is found during quiet respiration is not due to passive equilibrium of the muscles, but is due to the action of those which open the glottis.

We will first of all discuss that position of the vocal cords which is found after complete paralysis of the recurrent nerve, when the cords are in the cadaverous position; we then find the cords are nearer the middle line than is the case during quiet respiration, and also that they are in an absolute state of rest. The vocal cords cannot with the utmost endeavours be brought any nearer the middle line, owing to the absolute powerlessness of the muscles which bring together and tighten these bands; these latter therefore remain absolutely immoveable in this cadaverous position, and the production of any tone in the voice is impossible. And on the other hand, the separation of the vocal cords, which I have already told you is effected by the crico-arytænoidei postici, is impossible. In consequence

of this we get absolute aphonia, whilst, under ordinary circumstances, we do not find marked difficulties of respiration. With the vocal cords in the cadaverous position the cleft of the glottis is always sufficiently wide to meet the demands of ordinary respiration. But it is not so when the muscles which open the glottis are the only ones paralysed. The muscles which tighten and bring together the vocal cords are then unaffected by the paralysis, and consequently the glottis is closed without difficulty. *Therefore in uncomplicated double paralysis of the muscles which widen the glottis the voice remains intact.*

Seeing, however, as we have already shown, that we get the so-called cadaverous position of the vocal cords when the conductibility of the nerve is completely destroyed, and the muscles which both separate and bring together the cords are perfectly paralysed; it follows that the paralysis of those muscles only which separate the vocal cords will, at least primarily, be followed by the same result, with this difference only, that whilst in the first case the vocal cords are absolutely immoveable, no movement inwards or outwards being possible, in the second the vocal cords may be moved nearer to one another in the production of the voice, though they cannot be separated from one another beyond the cadaverous position.

Full paralysis of the recurrent with its simultaneous effect of loss of power on the part of the constrictor and dilator will give us the same aperture of the glottis, and the same respiratory position of the vocal cords as is found after simple paralysis of the dilator alone. The difference between the two kinds of paralysis consists in this only, that in the latter there is still a possibility of narrowing the glottis and producing the voice, but in the former this is lost; more than this, in the former the cord cannot be moved in any direction. So far we can theoretically form a clinical picture of the effects of paralysis of the dilator of the glottis.

Owing to the fact that double-sided paralysis of the dilator produces the same respiratory or cadaverous position of the vocal cords as double-sided complete paralysis of the recurrent, we should, under ordinary circumstances, expect to find just as little hindrance to the function of respiration in the former case as we do in the latter. But the circumstances are quite altered for both when there is much movement of the body or exertion of the

strength. The cadaverous position of the vocal cords does not accommodate itself to the increased demands for air, and under such circumstances we consequently get more or less dyspnœa.

From the foregoing theoretical sketch of double paralysis of the dilator of the glottis we see that, as regards hurried respiration, it is quite comparable with double paralysis of the recurrent, indeed of the two the former is the less serious affection, for with it we still have the voice, but in the latter we do not. So that respiratory troubles, which under ordinary circumstances are not marked, are much the same in the one case as the other.

If this description of the disease really corresponds with our clinical experience of it, we need not be surprised that it is often overlooked. These slight difficulties in the respiration unaccompanied by alteration in the voice are the more easily misinterpreted, because this affection not infrequently makes its appearance as a sequel of other diseases, more especially after diseases of the lungs, mediastinum, pericardium, etc. Indeed, owing to the voice being intact and that there is no marked dyspnœa, there may be nothing specially to draw the attention to the larynx as the seat of disease. Only when the condition of the other organs is absolutely negative does the probability present itself that it is with an affection of the larynx that we have to deal. And on the other hand, when we have simultaneous complications with other organs, especially those of the thorax, this disease is very likely to be overlooked.

Little as the few cases of this disease which have been reported agree with the above described unalarming and ill-defined malady, there is no doubt that the first stages of the disorder correspond with the description I have given. And although clinical observation does not agree with the picture I have drawn, and the reports say that the symptoms were sudden and alarming, this apparent contradiction can be easily explained. In the above described first stage of the disease the morbid symptoms are so little marked that it is quite conceivable they might be overlooked, or that, owing to the complications, they might be misinterpreted. But the scene soon changes, and, as we have found by clinical experience, at least in cases of pure complete paralysis of the dilator of the vocal cords, a short time after the commencement of th

paralysis the condition of the glottis changes essentially. At the same time the disease, which in its very early stage seems so innocent and free from danger, becomes all at once serious and anxious. And those violent and alarming symptoms which are met with in the pure cases of the disease, and which we shall have to consider more in detail later on, soon make their appearance.

*When the recurrent is completely paralysed the aperture of the glottis is always the same*, because the dilating and constricting powers are totally gone. The vocal cords under such conditions, no matter whether the paralysis is of long or short duration, always occupy the same position, owing to the fact that there is no agent by which they can be disturbed from it.

*It is very different with simple paralysis of the dilator*, when the numerous antagonistic muscles remain intact. If the paralysis of the dilator continues very long the strength of the intact constrictors begins to tell, and the vocal cords are gradually drawn inwards towards the middle line. In this way the vocal cords are, little by little, drawn from their former cadaverous position to one much nearer the middle line, until they finally occupy a more or less completely median position, and sometimes, as was shown in a case reported by me,* we find only a linear fissure between the cords to carry on the function of respiration. And it is then that we get those terrible and alarming symptoms which made their appearance in the case I have mentioned. This was the first reported case of complete paralysis of the dilator which had been confirmed by subsequent dissection. (Cases of less complete paralysis had been previously reported by Gerhardt and others.†)

This secondary dragging of the vocal cords towards the median line is so analogous to what takes place after other paralyses that we ought not to be surprised at its occurrence when this particular paralysis has continued for a longer or shorter period.

This phenomenon, which is more especially observed in the extremities, is called "*paralytic contraction.*" The median

* Ueber die Lähmung der Glottiserweiterer. Berliner klinische Wochenschrift. 1872. No. 20 u. 21. 1873. No. 7.

† Studien u. Beobachtungen über Stimmbandlähmung. Virchow's Archiv Bd. xvii. Heft 2. 3. u. 4.

position, which the cords gradually take up after paralysis of the dilator, is explainable in the same way as the extreme state of flexion of the upper extremities is, which comes on after paralysis of the extensors, such as we see, for example, after lead poisoning.

Owing to the fact that after paralysis of the dilator the constrictor and tightener of the vocal cords are alone able to contract, the action of the two latter easily becomes excessive. And owing to the want of an opposing dilator the cords, even after the action of the constrictor has ceased, remain more or less shortened and do not return to their earlier position. In this way we get, gradually, in the course of time, an ever-increasing constriction, until at last the cadaverous position is much exceeded and we finally get nothing but quite a narrow chink left between the two cords.

Now and then, when the cadaverous position of the cords has been shifted inwards, the glottis will become too narrow to supply the demands of even ordinary respiration. A sufficient quantity of air for quiet respiration cannot get through the glottis, and so, according as this stenosis is more or less acute, we get more or less severe dyspnœa. The voice however remains intact, because the function of the constrictor is in no way impaired. In this way there gathers together inevitably in the course of the affection a group of extremely violent and noisy symptoms out of what appeared to be a matter of so little moment.

The experiments which have been made upon animals have yielded results which support the points I have taken up. Schech's * beautiful experiments upon the functions of the nerves and muscles of the larynx are especially valuable in the evidence they have given. In addition to his inquiry as to the rôle of the various nervous and muscular elements of the larynx (which we will not stop to consider now), Schech endeavoured to show experimentally what the position of the glottis is after the action of the dilator has been excluded. I was present at the greater number of these experiments and can vouch for the accuracy of the results.

* Schech, Experimentelle Untersuchungen über die Functionen der Nerven und Muskeln des Kehlkopfs. Würzburg, Habilitationsschrift. 1873.

Taking into consideration the great technical difficulties which surround an experimental investigation of this question, it is not surprising that many of the experiments should have been futile and only a few have yielded positive results.

The evidence which was gained from his experiments upon dogs agrees entirely with the theoretical conclusions which have been drawn. Schech sums up the results of his experiments in the following terms:

"After division of both the dilators, the vocal cords pass beyond the cadaverous position inwards towards the median line and lose the power of separating on inspiration; the full median position and lasting dyspnœa do not immediately make their appearance; the power of narrowing the glottis and the vibratory functions of the vocal cords are undisturbed, whilst the voice remains intact."

As a rule, the operation is immediately followed by considerable dyspnœa, but this soon disappears. Schech says that this primary dyspnœa is the result of the constriction of the glottis which takes place immediately after the operation, which again necessitates deeper and more frequent respirations. In this way there arises a difference of tension in the column of air above and below the glottis, the vocal cords approach still nearer to one another, and thus the entrance of atmospheric air is still more impeded.

The abatement of the dyspnœa soon after the operation on animals is due, on the one hand, to the fact that the respiration gradually becomes quieter, and on the other to the fact that the animal soon becomes accustomed to the hindrance in the respiration; but it must be specially borne in mind that the constrictor of the glottis *never* exerts its power *suddenly*, but only after a certain period.

The dyspnœa and stridulous breathing make their appearance, however, immediately the animal attempts any violent exertion. But in none of these experiments was there any alteration of the voice, which corresponds with what I have previously told you.

If we now take up more closely the peculiar clinical condition in this disease, I shall have little to add to the description which I have, to a certain extent theoretically, given you. I

shall not include here an exact description of the symptoms in one-sided paralysis of the dilator, because it is so easy to conceive it from that of the symptoms in double paralysis. Difficult as it is to understand, the symptoms in this complaint are so little marked that they are likely to escape observation altogether. Sufficient reasons have been already given why the voice should be unaffected; and respiration is not much impeded even when the antagonistic action of the contractor is fully developed, because a sufficient quantity of air for ordinary respiration can always pass in and out of the merely one-sidedly narrowed glottis.

But this is not the case with double-sided paralysis of the dilator, which we will now discuss in more detail. Here, however, the symptoms vary very much, according as we have to do with the disease in its earlier stages, when the vocal cords still remain in the cadaverous position, or as in the further course of the disease we have sooner or later to do with a secondary contraction of the constrictor.

The preceding remarks show plainly enough that this secondary contraction of the constrictor does not always make its appearance at the same stage of the paralysis.

When the paralysis is complete the constrictor gets the upper hand in a comparatively short time and the respiration assumes the characters of what may be considered nearly typical croupous breathing. In incomplete paralysis, which one can easily imagine is the more likely to occur, the vocal cords naturally will not be drawn either so quickly or so strongly towards the middle line, because the remaining fibres of the dilator will still retain their activity.

Owing to the first symptoms of incomplete paralysis of the dilator being very little marked and easily escaping observation, the actual commencement of the affection is in most cases difficult to determine. Only in some few cases has the beginning of the disease been at all sharply defined, in one very interesting case reported by Feith * this occurred, here paralysis of the soft palate (Gaumensegel) and paralysis of the dilator of the glottis appeared simultaneously after an attack of erysipelas.

* Sitzung des allgemeinen ärtzlichen Vereins in Cöln vom 24. August 1874. Berliner Klinische Wochenschrift 1874. No. 49.

Only a few days after the disease had begun the impediment in the larynx became so great that tracheotomy had to be performed.

Therefore the rapidity with which the secondary contraction of the antagonistic muscles appears is always in direct proportion to the completeness of the paralysis.

Whilst now proceeding to an analysis of the separate symptoms, I shall have in my mind's eye a case of pure complete paralysis with already developed secondary contraction.

As a preliminary, I make the following proposition: *that the symptoms of double-sided full and simple paralysis of the dilator are so nearly characteristic that without the laryngoscope the existence of this form of paralysis may be pretty well ascertained.* I draw particular attention to this because it is not uncommonly thought that this condition of things is in almost all cases only accidentally produced and that there is nothing in the symptoms which would *a priori* denote more than the *possible* presence of this affection. The diagnosis will naturally only be one of probability without the help of the laryngoscope; it is important however to bear in mind that so much is possible without the laryngoscope. And this is the more necessary because such cases, owing to there being no difficulty of speech, do not, as a rule, go to the specialist, but to the general practitioner.

If we now analyse the more important symptoms *seriatim* we find that the *acute inspiratory dyspnœa* is the most striking and serious. I have already told you on various occasions, and cannot impress upon you too strongly, that we ought not to use the term dyspnœa carelessly, but that we ought to make a threefold division of it into *inspiratory*, *expiratory*, and *mixed*. I shall not at the present moment go into the special characters of the two latter forms, but shall proceed to recall to your memory the more important points in inspiratory dyspnœa.

The differences between those three forms of dyspnœa can no doubt be best demonstrated by means of accurate mechanical apparatus, as for instance by means of my *single* and *double stethographs*, with which I first graphically demonstrated their differences with precision, and the same can be done by means of *Waldenburg's Pneumatometer;* nevertheless such refined

instruments are not necessary for the above-mentioned purpose; the usual physical means are sufficient, indeed an attentive eye is quite enough.

The *special* advantage of the foregoing apparatus is that it not only accurately demonstrates the character of the particular form of dyspnœa, but that it also gives at the same time an accurate measure of the amount of inspiratory or expiratory dyspnœa. But for the purpose in question simple inspection is sufficient.

In all these cases respiration is very laborious owing to the marked constriction of the glottis. All or many of the inspiratory muscles are brought into action to overcome as much as possible the constriction of the canal by the increased employment of force. All the efforts employed, however, are not sufficient to carry the necessary quantity of fresh air through the narrowed glottis in a given space of time. Consequently the patient takes a longer time than natural for inspiration. At the same time, as I have shown in an exquisite example of this disease by means of the graphic method, the expansion of the thorax becomes less and less with each inspiratory movement.

Whilst, however, the act of *inspiration* occupies a much longer time than natural, owing to the impediment to the entrance of air, *expiration* is comparatively easy and quick. This is easily explained from the fact that the impediment which is thrown in the way of expiration is so much less important, also from the comparatively slight amount of dilatation which precedes inspiration even after the utmost straining, and also from the great force ready to be employed at the commencement of expiration.

Now it is precisely in this disproportion between the length of inspiration and expiration, and in the disproportion of the strength expended in the two acts that we find the peculiar characteristics of inspiratory dyspnœa, hurried respiration is by no means characteristic of it. Indeed respiration in the majority of cases is little or not at all hurried, and frequently it is rendered slower. We can easily understand how it is that respiration is comparatively so slow though the breathlessness is so extreme, when we bear in mind what a disproportion there is between the times allotted to the two acts of respiration.

We find an analogous though converse state of things in patients with impeded expiration, as for example in emphysema; in the former case we had inspiration prolonged at the expense of expiration, but here we have expiration prolonged at the expense of inspiration.

These phenomena are easily explainable according to Breuer's* investigations. For as he has pointed out it is the very essence of the self-acting functions of the vagus to correct impediments in the respiration by modifications of the mode of breathing. So if the lungs are imperfectly filled the inspiratory effort will be longer and stronger, and if the lungs are imperfectly emptied the expiratory effort will be lengthened and strengthened.

Besides this disproportion between the time and force employed in the inspiratory and expiratory efforts the breathing in the disease in question possesses still further peculiarities. As a rule, the breathing, as in all stenoses of the great air-passages, exhibits the peculiarity of the "forced costal type." Owing to the fact that, despite all the straining and help of a variety of inspiratory muscles, a proper quantity of air does not reach the lungs, it follows that the air within the lungs becomes correspondingly much rarified, and so, in advanced cases of this affection, with each inspiratory effort we get a recession of the walls at the most yielding portions of the thorax, more especially at the diaphragm, the lower portion of the sternum, and the adjoining cartilages. At the same time the intercostal spaces deepen and the jugular and clavicular regions sink in.

As a further point, and one which serves to distinguish tracheal from laryngeal stenosis, we may mention that in laryngeal stenosis, the larynx, in consequence of the difference of atmospheric pressure above and below the constricted part, moves up and down considerably during each respiratory act; whilst in tracheal stenosis this movement is either absent altogether or only present to a very small extent (Gerdardt†).

This symptom is of more especial importance with regard to the diagnosis of tracheal stenosis, because the symptoms mentioned previously are common to both tracheal and laryn-

* Die Selbststeuerung der Athmung durch den N. vagus. Sitzungsber. der K. K. Academie der Wissensch. zu Wien. Bd. lviii. Abth ii. Nov. 1868.

† Lehrbuch der Auscultation und Percussion. 2. Auflage p. 219.

geal stenosis. So that this symptom is an indication that the seat of the stenosis is in the larynx.

Further, the *intact condition of the voice is* of very high diagnostic value. This symptom at the first glance would appear opposed to an affection of the larynx, because the majority of laryngeal affections, whether they constrict the tube or not, usually cause more or less alteration of the voice; nevertheless the presence of this symptom and the one previously mentioned makes it very probable that we have to do with the disease in question, and excludes other laryngeal affections. This probability will be strengthened if the symptoms of laryngeal stenosis have become gradually developed and if one of the well-known causes of this disease is present.

With regard to the other symptoms of double-sided paralysis of the dilator of the glottis, I do not intend giving you a detailed account, I shall only say a few words about them. They are in reality similar to those which make their appearance in other forms of laryngeal and tracheal stenosis. They vary only according to the degree of stenosis, as is the case in stenoses arising from other causes.

The stenotic murmur, and high-toned panting respiration which we hear in the neighbourhood of the stenosis, belong to this group of symptoms. The stenotic murmur, which can be heard particularly well during inspiration, is often so loud that it may be heard at a tolerable distance off. As a rule, its loudness is in proportion to the intensity of the stenosis. The murmur can not uncommonly be felt as a thrill externally and the site of the stenosis localised to a certain extent.

In acute cases the vesicular murmur can scarcely be heard, especially in the lower portions of the thorax; at one time it is considerably weakened, and at another completely drowned by the stenotic murmur.

In uncomplicated cases there is never any cough or expectoration. Nor does the temperature ever rise unless some complication sets in. The pulse, sometimes quickened, is mostly small and soft. The appearance of the patient corresponds to the imperfect respiration—owing to the imperfect decarbonisation of the blood the face has a more or less livid hue; and the peripheral portions of the body, *i.e.* the hands

and feet, feel cold. When the patient walks about the breathlessness becomes most marked.

The subjective symptoms are evidently the same as we see in all cases of well developed laryngeal or tracheal stenosis, and they are most marked in those cases where we have a further, though slight, affection superadded. Thus in a case of this kind reported by me, a slight amount of laryngitis sufficed to call forth the most acute symptoms of suffocation, which could only be relieved by the immediate performance of tracheotomy.

Thus whilst the greater number of the foregoing symptoms are common to both laryngeal and tracheal stenosis, the marked up-and-down movement of the larynx during respiration, and the intact condition of the voice make it very probable that double-sided paralysis of the dilator is the cause of the stenotic symptoms. Almost all the other laryngeal affections which give rise to marked stenosis are accompanied by considerable change in the voice. But absolute certainty in the diagnosis can only be gained by means of the laryngeal mirror.

If we examine a marked case of uncomplicated double-sided paralysis of the dilator which has been followed by the secondary contraction of the constrictor, such a case, for example, as that most marked one which was reported by myself, we find *the following state of things with the laryngoscope:*

If there are no complications we find all the structures of the larynx perfectly normal as regards colour, size, and relations. It sometimes happens, and it is specially likely to happen in those cases where the epiglottis is much notched on each side or topples back far posteriorly, that a view of the interior of the larynx is difficult to obtain; it is particularly likely to be the case with patients who are unused to such examinations; under such circumstances we can only see the larynx during intonation, and do not by any means discover what the disease is like. It is quite evident that to obtain a proper knowledge of this affection it is necessary to watch the larynx during the act of respiration, because the difficulties which have been mentioned occur at such time only. Seeing that the vocal functions of the larynx remain intact, we can quite understand that if the larynx be watched with the mirror during the act

of intonation alone, the affection may be completely overlooked. In all laryngoscopic investigations it is necessary, but in this affection it is especially necessary, to watch the condition of the larynx during the act of respiration. It is then seen that during quiet respiration the two vocal cords are so close to one another that only a narrow, almost linear chink is left between them. At the same time during inspiration, especially during forced inspiration, we see that the vocal cords approach one another so closely as to produce almost complete closure of the glottis. It is only by the closest observation that we can find any interval between them. During each act of expiration the cords separate somewhat, though it may be but very little. The more violent the act of inspiration is, the closer the vocal cords are applied to one another. During intonation, on the contrary, when the voice is raised the two vocal cords and arytenoid cartilages separate from one another in a perfectly normal manner and vibrate quite naturally, which shows that the constrictor and tightener of the vocal cords still retain their proper function.

As I have now pointed out the main features in the clinical history of this disease, I must expressly tell you that the description I have given only corresponds to the pure, well marked, and uncomplicated forms of the affection. I have previously told you distinctly, and I repeat it again now, that, as might be expected on theoretical grounds, *recent* paralysis of the dilator gives rise to the same position of the cords as double-sided complete paralysis of the recurrent. The before-mentioned experimental observations have shown the correctness of this statement. The former can be distinguished from the latter at this early stage by this circumstance alone, that after paralysis of the recurrent, movement of the cords both inwards and outwards is totally wanting, but after paralysis of the dilator only the power separating the cords is wanting. Consequently in the early stages of this affection, as well as in the more frequent incomplete forms of the paralysis, the above-described symptoms of stenosis are either undeveloped or present to a very slight extent. But they gradually increase, and though the voice remains perfect they become more and more intensified. As a rule, as the secondary antagonistic contraction of the constrictor becomes more and more developed, so the chink of the glottis

becomes narrower, until at last that final acute stage mentioned above is reached. That this stage may be reached in a very short time is shown by numerous observations, and more especially by Feith's case, which I have mentioned to you before; in this the commencement of the affection was pretty accurately determined. But in many cases it is impossible to say when the disease begins, because the first symptoms are so insidious that they escape observation.

The further course of this disease is different in different cases. In many, especially in the ill-developed cases, the symptoms of stenosis of the glottis may go on for years, and with care the patient may get along pretty well. This was the case with a patient of Gerhardt's, in whom the disease existed unchanged for twenty-eight years. In other cases again the dyspnœa becomes so acute that tracheotomy has to be performed at once to save the life, and this is especially the case when a complication in the shape of some other simple affection, such as laryngitis, tracheitis, or bronchitis is superadded. After the tracheotomy the patient feels quite secure again; but the canula must be kept in position lest symptoms of suffocation should appear again, provided of course the paralysis itself cannot be cured.

In other cases again, in those, for instance, where there is but a temporary cause for the disease, where it has an hysterical foundation, the affection gets well by itself, or yields after a longer or shorter time when the remedies which I shall have to mention to you are employed.

In still other cases it is not so much the paralysis as the original disease which brings about the death of the patient, as, for instance, when an aortic aneurism, a mediastinal tumour or disease of the apices of the lungs, presses upon the recurrent and destroys its functions. In these cases the fatal result is brought about by the increase of the original disease.

With reference to the ultimate issue of this affection, I must tell you that in the majority of cases the disease is not cured, much more frequently the disease drags on unchanged for a long time, or death takes place owing to the advance of the original cause of the mischief. The few cases of this disease which are reported to have been cured are by no means free from suspicion as to the correctness of the diagnosis. In any

case there is no doubt that in the majority of cases cure does not take place.

To conclude what I have told you with regard to the symptomatology and course of this disease I will, with your permission, describe a recent case I have met with; here you will see that the real symptoms, though much obscured by the numerous complications, presented so many characteristic points that they stood out in bold relief amidst the other symptoms.

The case occurred in a man 58 years old, who for a long term of years had acted as verger in our cathedral. The patient was admitted into the hospital at the beginning of December, 1874, with symptoms of extreme dyspnœa. The history showed that with the exception of a few uninteresting intercurrent affections in his youth the patient had always had good health. His present disease, which dated back to the beginning of November, was attributed to the great draught at the doors.

The illness he was suffering from had gradually developed itself, and the first symptom he had noticed was an increasing shortness of breath, which however at first inconvenienced him so little that for a short time he could still continue at his duties. But soon afterwards breathing became more difficult and he was obliged to keep to his room. In addition he gradually got a violent cough, with pretty copious mucous expectoration and some hoarseness. How long the cough had existed could not be gathered from our not very intelligent patient, for his statements were by no means always to be depended upon. During the last few days the shortness of breath had so much increased that the patient saw it was necessary to come to the hospital.

On his admission the following state of things was found:

High-toned, long-drawn inspiration audible at a great distance, with comparatively short and easy expiration. The larynx moved downwards markedly with each act of inspiration. The voice rough, but free from clang. Externally the larynx was unchanged. The rate of respiration, notwithstanding the extreme dyspnœa, was not increased. With each act of inspiration, which was performed with the utmost effort, a slight furrow was formed at the lowest portion of the sternum and in the epigastrium, which began to be restored towards the end of inspiration.

By percussion, the upper portion of the right thorax, both anteriorly and posteriorly, was found to yield a note considerably shorter and duller than the left. This dulness reached down anteriorly to the third rib, and posteriorly to a finger's breadth below the spine of the scapula, below these points the resonance became gradually restored. Everywhere else both anteriorly and posteriorly the lung-note was good.

In the region of the dulness there was bronchial breathing with a good many medium and small-sized bubbling râles. There was no change of note when the mouth was opened or shut. The sputa was partly frothy, partly muco-purulent, with here and there quite superficial streaks of blood adhering to it.

There was no change in the heart; the pulse was regular, large and full, certainly not increased in frequency. The temperature was normal. The urine was cloudy and yellowish, with a trace of albumen.

The result of this examination, although made immediately upon the admission of the patient and showing some positive changes, could not be considered by any means satisfactory. The comparatively slight amount of thickening discoverable in the upper lobe of the right lung could not explain the extremely marked dyspnœa of our patient. On the contrary, the peculiar character of the dyspnœa, let alone the alteration of the voice, pointed with certainty to some impediment in the larger air-passages, larynx or trachea. The intact voice, the special character of the dyspnœa, with the difficult, long-drawn, high-toned inspiration and short expiration, both pointed strongly to an impediment in one of the larger air-passages. There was another symptom which, without a laryngoscopic examination, made it very probable that the impediment had its seat in the larynx, and not in the trachea. This was the great movement of the larynx during each act of respiration. This, as you know, is a strong point of distinction between laryngeal and tracheal stenosis.

We were thus enabled to say without the help of the laryngoscope that there was a change in the larynx which was accompanied by considerable stenosis of the tube. But what the nature of this was we could not find out without some other means of diagnosis. There might be a considerable swelling and thickening of the vocal cords, or some large new-growth, or perhaps perichondritis, or paralysis of the dilator of the

vocal cords or the like; paralysis of the cords being the least likely, owing to the simultaneous though slight change in the voice. Naturally this point could only be decided with the laryngoscope.

A laryngoscopic examination was immediateley made and gave a most satisfactory explanation:

During respiration there was only a very narrow cleft to be found between the vocal cords, which on inspiration became still narrower, and on expiration returned to its previous condition. The right vocal cord was normal as regards colour, size, etc.; the left was slightly reddened, and at its free border towards the back exhibited a small superficial erosion; also the free border of the left false cord (Taschenband) was slightly ragged. There was no special swelling of these parts. When intonation was attempted, the voice was somewhat rough, but by no means wanting in tone, and the two vocal cords met completely in the middle line, but in the inspiration which followed they only separated a very little from one another.

Therefore the laryngoscope revealed two totally distinct forms of disease; in the first place, there was a laryngitis limited to the left vocal cord, but involving also a part of the left false vocal cord and accompanied by some superficial erosion, an affection having some connection, perhaps, with the infiltration at the apex of the right lung. In the second place, there was an extremely well-marked stenosis of the larynx, due to the very near approach of the two vocal cords to one another. The vocal cords could easily be brought together at the middle line, as was shown during intonation; but they could not, even on the deepest inspiration, be separated beyond the foregoing extremely close position, which was far from equalling in width the cadaverous position of the vocal cords. Owing to the absence of all mechanical impediments this defective separation of the vocal cords from one another could only be explained by a paralysis of the dilator of the vocal cords, and a double-sided one too.

It was quite evident that the laryngitis with the superficial ulceration of the one vocal cord could not be taken as a cause of this marked approximation of both vocal cords to one another. For in the first place there was not much swelling, and in the second what slight redness there was was limited to the left

cord, whilst the want of movement existed in an equal degree on both sides.

I shall only very shortly describe the further course of the case.

Towards the evening of the day after admission the dyspnœa increased to so alarming an extent that we were compelled to perform tracheotomy, for which the patient had been prepared immediately after the laryngoscopic examination. Complete subjective euphoria was the result, but it only lasted for a few days. There was soon slight feverishness, and the dyspnœa became again more marked, without however by any means reaching its former intensity or showing that peculiar character in the inspiration which I have mentioned. The dulness at the apex increased, the ulceration on the true and false vocal cords, at first quite superficial, now became deeper, and simultaneously the cords became slightly swollen. At the same time the tracheal wound, which at first looked healthy, gradually became a nasty colour. The patient became correspondingly prostrate and finally died in a state of extreme collapse.

With regard to the post-mortem examination, I shall mention those points only which bear upon the question we are considering.

The left lung was everywhere well filled with air, on the cut surface only some isolated small, hard, infiltrated and somewhat prominent masses presented themselves, which mostly exhibited small cheesy nodules; these masses were fixed in part to the bronchi. The middle and lower lobes of the right lung were quite free, but the upper had grown tightly to the sternum. The tissue of the upper lobe was almost uniformly slate-like in colour and indurated, but in the middle there was a pretty large cavity, into which two large bronchi opened; behind these were some small isolated cheesy masses. The middle and lower lobes were normal. The bronchi were filled with a slimy purulent material and were much reddened.

The tracheal mucous membrane was very much reddened and somewhat thickened, and covered with a tenacious slime. The laryngeal mucous membrane, especially that of the vocal cords, was slightly thickened. On the left vocal cord there was a somewhat irregularly-shaped ulcer of the mucous membrane, and a similar superficially ulcerated spot on the left false vocal cord. The larynx was otherwise unchanged.

Neither vagi had undergone any microscopical change so far as the recurrents.

When the larynx was carefully examined, all the muscles with the exception of the two crico-arytænoidei postici were found to be quite healthy; these two had a most striking white and stringy appearance, and seemed to be almost destitute of muscular tissue, they were in fact in a high state of atrophy.

When examined microscopically much fibrous tissue was found between the muscular fibres. In only a small part was there a fair quantity of muscular tissue, and here the transverse striæ were ill-marked, and in parts the tissues were undergoing granular degeneration. The other laryngeal muscles were quite intact. Microscopical examination of the vagi and recurrent did not reveal the slightest change anywhere in their course.

The results of the post-mortem examination, therefore, proved the correctness of our diagnosis. The clinical symptoms proved beyond a doubt that the marked dyspnœa could be the result of nothing but double-sided paralysis of the dilator of the glottis, for there were no other pathological symptoms in connection with the larynx, except the slight laryngitis. And the latter, especially at the time of admission, when the dyspnœa was most acute, was so insignificant that it is impossible it could have caused more than a slight stenosis, let alone that marked approximation of the two vocal cords to one another.

Although the autopsy had established the diagnosis beyond a doubt, there was still another by no means unimportant question which remained to be settled, the question as to the origin of this form of paralysis. It might be either neuropathic or myopathic, as far as the evidence went. And although positive facts were wanting I was more inclined to think that it was the former, having seen a very similar case previously. In that, which exhibited double-sided paralysis of the dilator in its purest and most extreme shape, the cause of the affection was not discovered with certainty during life. After death, however, we found both the recurrents partially atrophied from pressure, on the right side at the apex of the lung, on the left at the aortic arch.

In the present case, however, the autopsy did not reveal the cause of the paralysis and atrophy of both the crico-arytænoidei

postici. The case, however, is particularly interesting on other grounds; more especially because in spite of the haze thrown around it by the complications, there was a possibility of making a safe diagnosis.

Unfortunately there is very little known as to the etiology of this disease. We can easily understand why the number of accurately reported and well established cases is very small. I say again, we can easily understand it, because it must be one of the rarest occurrences for just those fibres, and just those fibres only, of the recurrent which supply the crico-arytænoideus to be paralysed. Again, if the paralysis be of myopathic origin, it must without doubt be a rare thing for both the crico-arytænoidei to become unfit to perform their proper functions whilst all the other muscles remain intact. In this direction then it is quite evident we have by no means fathomed the etiology of this disease.

The case recorded by Gerhardt, to which I have before referred, was the first example of the disease which was published. This occurred in a physician, who was positive that his malady dated from a chill which he had got 28 years previously. Catarrh and hoarseness followed and the breath was short ever after. There was no organic disease. Catarrh was apparently the starting-point of the paralysis in this case; as you know, it is one of the commonest causes of paralysis of the laryngeal muscles. *Catarrh* being regarded generally as a cause of laryngeal paralysis, we may consider that it was one of the original factors in this case. No post-mortem examination having been made, it is of course impossible to say positively whether there was another cause at work.

*The conductibility of the recurrent or vagus may be injured* and be a further cause of paralysis of the dilator. The same diseases which now and then entirely destroy the function of the recurrent, especially mediastinal tumours, aortic aneurisms, pericardial exudations, affections of the apex of the right lung, etc., may also only partially impair the conductibility of this nerve. In a similar manner the special twig of the recurrent which supplies the dilator may have its functions destroyed.

There can be no doubt also that when the mischief is in the trunk of the recurrent only certain of its fibres may have their

conductibility destroyed, and under some special circumstances it is those only which supply the dilator.

It is *a priori* easily seen that when the entire trunk of the nerve is exposed to pressure, some of the nervous elements will be much, some slightly, and some not at all compressed. The result of the autopsy in a case which I published gave evidence that this had occurred. The patient, a boy six years old, was admitted with the most extreme inspiratory dyspnœa, but with uninjured voice. A short time after the performance of tracheotomy he died of an intercurrent affection.

A post-mortem examination showed that both recurrents, the right above the apex of the lung, and the left above the aortic arch between the trachea and œsophagus, had become enveloped in dense connective tissue; many of the nerve-fibres had atrophied and undergone fatty degeneration, others were normal; the crico-arytænoidei postici were the only muscles which were markedly atrophied.

Just as the vagus is affected at its periphery so may it and the accessory nerve, which latter indeed supplies the motor fibres to the laryngeal nerves, be found to sympathise either wholly or in part with disease of the central organ, bulbar paralysis, etc. One-sided paralysis of the dilator has been many times found in connection with a central cause. A case of double-sided paralysis of the dilator belonging to this group has been lately reported by Penzoldt. The autopsy showed that both the vagi were strikingly thin and grey at their roots; and the same was the case with the accessories. Both the posterior crico-arytænoid muscles were of a pale brownish red colour. Here, evidently, the atrophy of the recurrent, making itself apparent in the accessory and vagus, was primary, and the softening degeneration of the muscles secondary.

Infectious diseases, especially diphtheria, the acute exanthemata, erysipelas, and the like, are, I think, not infrequently the cause of paralysis of the dilator. It is not surprising that out of the small total number of cases recorded those traceable to this source should be exceedingly rare. A case belonging to this group which I had the opportunity of observing was recorded a short time since by Feith.* It occurred in a stout female aged 68 years, who got an attack of erysipelas in the

* Berliner klinische Wochenschrift 1874. No. 49.

face. On the third day of the disease there was a considerable splenic tumour. On the ninth day the fever abated, but on the following day there was an exacerbation, and pneumonia on the left side now showed itself. At the same time difficulty in swallowing, due to paralysis of the soft palate, made its appearance; and in a few days there was added to this, whilst the voice was still intact, an ever increasing inspiratory dyspnœa. The laryngoscope revealed a considerable approximation of both vocal cords to the middle line, more marked on the left than the right. At the same time there was the normal closure of the glottis during speech, and the voice was unchanged. Even on deep inspiration the vocal cords would not move outwards to the respiratory position, on the contrary, they got closer and closer together until only a linear cleft was left between them.

Although paralysis of the soft palate, etc., after infectious diseases is by no means uncommon, this is the only well authenticated case of respiratory paralysis following erysipelas.

Also paralysis of the dilator as a sequence of diphtheria, compared to other paralyses from the same affection, has been hitherto very seldom seen. Whether Turk's * case of so-called diphtheritic paralysis, in which the vocal cords separated during ordinary respiration, but almost touched during quick, deep inspiration, deserves to be so designated, is at least open to question. Also by some other authors, Oertel and others, the occurrence of this form of paralysis after diphtheria is mentioned; nevertheless the majority of these cases cannot be reckoned as simple and complete examples of paralysis, for it is expressly mentioned that the respiration was never at any time markedly disturbed.

I cannot help thinking that many of the patients suffering from diphtheria, upon whom tracheotomy is performed, are not operated upon so much on account of the diphtheria as on account of a secondary paralysis of the dilator. Only a short time since I saw a case of this kind, though it was after the tracheotomy had been performed certainly, in which, according to the account of the medical man in attendance, all symptoms of diphtheria had disappeared for about eight days when

* Klinik der Krankheiten des Kehlkopfs und der Luftröhre 1866. p. 461.

dyspnœa made its appearance, and finally became so acute that it was necessary to perform tracheotomy. When I saw the patient the paralysis of the dilator was fully developed, and it was impossible for the patient to breathe for more than a short time, when the canula was stopped up. It looked very much as though the paralysis was the real cause of the dyspnœa and subsequent tracheotomy.

The fact that paralysis of the dilator was only discovered a short time after tracheotomy had been performed suggests a theory with regard to this form of paralysis similar to that which was brought forward by Hüter* in connection with many cases of laryngeal stenosis, which he had observed after the performance of tracheotomy. Hüter thinks that sometimes this paralysis of the muscles of the vocal cords only comes on after the aërial stream has been for some time directed through the opening in the trachea.

Owing to the tracheotomy the action of the laryngeal respiratory muscles is suppressed, air no longer passes through the glottis, the dilator of the glottis becomes gradually paralysed, and the antagonistic constrictors gain the ascendency.

It is scarcely possible that this could have occurred in the foregoing case, because the paralysis was set up only a short time after the tracheotomy. But under any circumstances, if this mode of origin is to be applied to other cases, it must be one of the rarest occurrences, and the paralysis could only make its appearance after a long employment of the canula. There are plenty of cases in which after the continued employment of the canula no such paralysis has made its appearance, therefore it would not be consistent to trace its origin to this source alone.

And if this explanation of the origin were correct there would be no difficulty in preventing the paralysis taking place. For by using a canula with an aperture at the upper part a portion of the atmospheric air might be made to pass through the glottis, and the paralysis would be prevented.

*Hysteria* has also been considered of some etiological importance in the cases which have been published.

* Handbuch der allgemeinen und speciellen Chirurgie von Pitha und Billroth iii. Band. Erste Abthlg. 5. Liefer. 1872. Hüter, Tracheotomie und Laryngotomie. p. 95.

Thus, hysteria was evidently at the bottom of the affection which was reported by Biermer * as paralysis of the dilator, and the same was at the root of the second one lately published by Penzoldt.† Nevertheless it is open to question whether these two cases ought to be considered as belonging to the group that we are discussing. For it appears to me that the occurrence of sudden detached paroxysms is not unimportant evidence against such an interpretation. I shall however revert to this point more particularly when speaking of the differential diagnosis of this affection. In any case it is quite certain that though hysteria is a very common cause of phonetic paralysis, the dilator of the glottis is very rarely affected in hysterical persons. Whether the specially protected anatomical position of this muscle, and, in comparison with the other laryngeal muscles, the more reflective character of its action, contribute to this special exemption, I will not stop now to inquire.

Lastly, I must tell you that in many cases we can find no satisfactory cause. If we will but divest ourselves of our own arbitrary theories, we must admit that in many cases the disease has developed without any discoverable cause.

I have now mentioned the most important etiological factors of this disease, but I do not think that the etiology of this disease can be considered complete. So small a number of observations must leave many questions to be decided in the future.

With regard to the *differential diagnosis* of double-sided paralysis of the dilator and other affections I have not much to say.

It is self-evident that it is the *laryngoscopic examination alone* which justifies us in forming a positive diagnosis. But still, as I have before told you, we can at least reach a certain amount of probability as to the presence of this affection by the consideration of the remaining symptoms. Nevertheless a laryngoscopic examination is absolutely essential for a positive diagnosis.

* Ueber Bronchialasthma. Volkmann's Sammlung Klinischer Vorträge No. 12.

† Über die Paralyse der Glottiserweiterer. Deutches Archiv f. Klin. Med. Bd. xiii. p. 117.

But in spite of this, even when the examination has been often repeated, the disease may be confounded with many other affections, indeed in many cases the diagnosis presents very considerable difficulties.

Only the most superficial examination could lead one to mistake complete paralysis of the recurrent for the affection we are discussing. As Mackensie* has pointed out, even when the crico-thyroid muscle, which is supplied by the superior laryngeal nerve, has lengthened and narrowed the glottis, even then, when the stenosis of the glottis is strongly marked, it is not easy to make a mistake between complete recurrent laryngeal paralysis and double-sided paralysis of the dilator. We could only imagine such an occurrence possible when the laryngoscopic appearances of the two diseases pretty well resembled one another. At the first glance such a mistake might occur, because recurrent paralysis, under such circumstances, might cause considerable dyspnœa. But, as I have before said, we have a peculiarity which always distinguishes the one affection from the other; in complete recurrent paralysis the glottis is never quite closed, and there is aphonia. In double-sided paralysis of the dilator the glottis can be closed quite normally and the voice is intact. A mistake between these two forms of paralysis is, therefore, not possible if we will but bear in mind that most important point in their diagnosis—the capability of closing the glottis and moving the vocal cords during intonation.

But in many cases it is much more difficult to distinguish *spasm of the glottis* from the disease we are considering. Of course we could not make such a mistake where the disease had begun gradually and had got to its height bit by bit and remained there, as in those typical cases I have given you. No one, for instance, would have thought of laryngeal spasm in that case of Gerhardt's, in which the disease continued unchanged for twenty-eight years, nor in those other cases I have mentioned to you.

On the other hand, we have several cases on record in which the symptoms did not pursue the regular course of gradually developing themselves, but arose suddenly and disappeared in

* *Med. Times and Gaz.*, 1869, April 3rd.

a comparatively short time. This is not the place to go more particularly into these cases to weigh the question whether the writers were justified in saying that these cases were paralysis of the dilator, and not laryngeal spasm. Just now I shall only mention those special points which are in favour of laryngeal spasm and antagonistic to paralysis of the dilator, or the reverse.

*A priori* it certainly cannot be disputed that paralysis of the dilator may possibly originate in hysteria in a similar manner to other paralyses of the vocal cords. And if we grant the reality of rheumatic paralysis of the vocal cords we can conceive that such paralysis may be confined to the dilator alone, just as it is when derived from other sources.

When however in cases of this kind symptoms of extreme dyspnœa arise quite suddenly, and a laryngoscopic examination reveals such marked approximation of both vocal cords to one another that there is nothing but a linear cleft between them, whilst the power of separating them is lost, and these are published as cases of paralysis of the dilator, I must say I entertain the gravest doubts as to this being a correct interpretation of them.

I have before drawn your particular attention to the fact that double-sided paralysis of the dilator gives rise primarily to exactly the same position of the vocal cords as full recurrent paralysis does, *i.e.* the cadaverous position. The difference between the two forms consists in this alone, that in the former the vocal cords can still be brought together, whilst in the latter both inward and outward movement is lost. The action of the antagonistic contractors only gradually developes itself, and there cannot, at least primarily during quiet respiration, be marked dyspnœa.

How long it is before the secondary contraction shows itself depends upon different factors, thus upon the completeness of the paralysis of the dilator, etc. And Schech's experiments have shown incontestably that this secondary contraction never comes on suddenly. In the few clinical observations where it was possible to note the onset of the paralysis it was proved that the antagonistic contraction never developed itself immediately, but was always preceded by a long interval. It was after this that the dyspnœa became so extreme.

Those cases in which without any warning sudden and

extreme dyspnœa occurs, and in which a very slight aperture between the closely approximated vocal cords is found by means of the laryngoscope, are undoubtedly cases of laryngeal spasm, not cases of paralysis of the dilator.

Otherwise every case of laryngeal spasm might be at once put down as a case of paralysis of the dilator with secondary contraction of the antagonistic muscles. The clinical laryngoscopic symptoms of the two affections when at their height do undoubtedly resemble each other. Only the history of each marks the difference between them.

In paralysis of the dilator there is a progressive increase of the symptoms, a gradual growth of the dyspnœa. This occurs even when, as is by no means always the case, the paralysis is rapidly developed, because the secondary contraction is always a gradual process and increases slowly. I believe that the sudden onset of symptoms of stenosis, which must not be confounded with the sudden onset of the paralysis, always excludes the possibility of paralysis of the dilator. Nevertheless it cannot be questioned that laryngeal spasms may develope gradually and increase at a comparatively slow pace.

The simultaneous occurrence of cramp in other muscles is a further though less important symptom of laryngeal spasm. But it is never absolutely trustworthy, for cramp of the larynx and paralysis in other muscles by no means rarely occur simultaneously in hysterical persons. Thus a paralysis of this or that group of muscles with an hysterical foundation may occur simultaneously with cramp in other muscles, more especially in those of the glottis. Thus in any given case of this kind antecedent symptoms of phonetic paralysis of the vocal cords are by no means antagonistic to laryngeal spasm, or favourable to paralysis of the dilator; it is, on the contrary, not an unusual occurrence in hysterical persons, for laryngeal spasms to alternate with phonetic paralysis. It was only the other day that I saw a patient of this kind who perhaps for a whole day would be voiceless from paralysis of the vocal cords and then be suddenly attacked with acute dyspnœa in consequence of spasms in the glottis. In the latter case we often get clonic spasms of other muscles, especially the sterno-cleido-mastoid, superadded. This alternation of spasm and paralysis is not in the least surprising; indeed, on the contrary, we can easily imagine how a

cramp-like condition may relax in the course of time, and how, owing to the exhaustion of the muscles the opposite condition—paralysis—may take its place.

I can see no evidence in favour of the view taken by some authors, that when there has been an antecedent phonetic paralysis and subsequent symptoms of stenosis the latter are always due to a paralysed condition of the dilator, and that laryngeal spasm is, *a priori*, excluded.

Penzoldt* has lately brought out a new method of diagnosis. He was watching a case in which during quiet respiration both vocal cords had generally a "median position," and he noticed that when pressure was made upon the tolerably well-marked *struma* of the patient, "the vocal cords first narrowed the glottis and then closed it altogether." He believes that in difficult cases compression of the neck may be employed as a means of diagnosis, inasmuch as moderately developed symptoms of paralysis are thereby increased, and he is of opinion that in the foregoing case the paralysis of the dilator was increased by the experiment.

Strictly speaking, of course we cannot speak of strengthening a paralysis of the dilator, but only of increasing the spasm of the contractors, seeing that the constriction of the glottis is brought about by their action alone. But bearing in mind the fact that, normally, stimulation of the nerve generally, more especially electrical stimulation, by no means brings about the cadaverous position of the cords, as one might expect, but a phonation-position, I cannot look upon this as a fresh help in the diagnosis of paralysis of the dilator. Seeing that this is the *normal* reaction of the glottis under stimulation of its nerves, it cannot be also used as a means of diagnosis in the disease we are considering. The only difference to be noted perhaps is, that in the latter case a spasm might be developed by an irritant which would not have been able to do the same in the former. It therefore appears to me that this symptom is of very slight importance in the diagnosis of laryngeal paralysis from laryngeal spasm, so long as its value remains unconfirmed by further observations.

The persistence of the symptoms of stenosis is of just about as little value as the foregoing in drawing a distinction between

* Deutsches Archiv f. Klin. Medicin. Band xv. p. 604.

laryngeal spasm and paralysis. Laryngeal spasm reaches a very different degree of intensity in different cases; sometimes indeed in very acute cases the patient can only speak in a whisper. Although, as a rule, there is a complete intermission in typical laryngeal spasm, yet it is by no means uncommon for one paroxysm to last an hour. Indeed in severe cases sometimes the spasm will last for days with slight intermission. If anyone is inclined to doubt this, and thinks it impossible that a spasm of the glottis could last for days, let him bear in mind that in the interpretation of such cases as paralysis of the dilator we assume the possibility of this long persistent spasm. For it is only in this way, as I have again and again told you, that the intense symptoms of laryngeal stenosis are brought about, and not by the paralysis as such. That spasm of the larynx may continue without intermission for days is proved by the fact that pure acute cases of paralysis of the dilator are accompanied by persistent antagonistic contraction of the constrictor.

That sex, age, and the other circumstances of the patient may sometimes be of value in the discrimination of the two forms from one another I need scarcely mention. These factors in the diagnosis cannot however be mentioned in comparison with those we have previously considered.

I have now really come to the end of the diseases which can possibly give rise to errors in the diagnosis of paralysis of the laryngeal dilator. I have not thought it necessary to specially mention here those points which distinguish this disease from mechanical impediments, such as perichondritis, cicatrices, etc. The circumstance that these affections are scarcely ever symmetrically double-sided prevents any confusion. But the case lately reported by Sidlo* shows how difficult the diagnosis is sometimes; the patient was believed to be suffering from paralysis of the dilator; after death some atrophy of both crico-arytænoidei postici was found certainly, but the cause of the inability to contract on the part of the two dilators was then discovered to be an unsuspected cicatrix on the posterior wall of the larynx.

So far as one can judge from the few observations hitherto

* Wiener med. Wochenschrift 1875. N. 26. 27. 29.

published, the *Prognosis* in pure, well-defined cases is mostly unfavourable. The essential points in the prognosis are to be found in the factors of the case you are considering. Where the cause is not removable the prognosis is always an unfavourable one. Thus when that portion of the recurrent which supplies the dilator has degenerated in consequence of the contraction of cicatricial tissue, or owing to affections at the apex of the right lung, or aneurisms of the subclavian, aortic arch, or the like, we cannot expect recovery, as a rule. The same holds good when the affection starts from the central organ, when the vagus and accessory nerves are involved in a pathological process at their roots. As the causes of the nervine degeneration are almost always irremovable, the cure of the paralysis cannot be anticipated.

The prognosis is favourable in those cases in which hysteria is at the root of the affection, in the same way as it is in phonetic paralysis of the vocal cord when of like foundation. Nevertheless I must impress upon you the necessity of special care in the diagnosis of hysterical paralysis of the dilator.

Similarly the prognosis in the so-called rheumatic paralysis ought not to be an unfavourable one. But here also the affection may go on unchanged for years, as was shown in Gerhardt's case, in which it was plain enough at all events that the affection dated from a chill and that it went on unaltered for many years in spite of all the medicaments that were employed.

The prognosis of diphtheritic paralysis of the dilator of the glottis is comparatively favourable, as is that of the paralyses generally following infectious diseases. Paralysis of the dilator mostly follows the same course as the other paralyses do which make their appearance after these diseases. Nevertheless I cannot offer you evidence upon this point. In Feith's case, which I mentioned previously, an attack of erysipelas was followed simultaneously by paralysis of the soft palate and paralysis of the dilator—the paralysis of the soft palate soon yielded under treatment by electricity; but the paralysis of the dilator continued unchanged for almost a year in spite of all manner of remedies, and then death followed from an intercurrent affection.

In the majority of pure cases, as far as we know, recovery does not take place; of the few cases which do recover, some are incomplete and some admit of questions as to the diagnosis.

The *Treatment* of this affection is not, on the whole, very successful. Of course it varies with the cause of the affection. Thus in cases where the nerve is paralysed by the pressing and stretching which it undergoes from mediastinal tumours, aneurisms, contracting cicatricial tissue, or the like, and the cause of the disease is irremovable, it is impossible to attack the disease at its source. When it is possible to remove the original cause of the disease, of course we attack this first, for if this be removed and the nervous fibres have not degenerated, the paralysis will easily disappear. Thus in cases where the conductibility of the nerve is destroyed by the pressure of a struma, the first task is to remove the pressure. In those cases, on the contrary, where there is no possibility of removing the source of the mischief, we can only fall back upon a symptomatic treatment.

There is no doubt that amongst the different methods of treatment acting directly upon this form of paralysis, electricity plays the most important part. Either the Faradaic or the constant current may be employed, but the latter is not to be recommended. Some prefer the percutaneous Faradisation of the inferior laryngeal nerve, whilst others are in favour of applying the current directly to the posterior crico-arytænoid muscle. The latter method is to be preferred; the operation is performed by slipping the electrode down the posterior surface of the arytænoid cartilages to the posterior flat surface of the cricoid cartilage. As Ziemssen mentions, we often find considerable resistance at the constrictors at first, and have to twist the electrode about in order to get it down to the right spot. According to Ziemssen,* also, the electrode may be carried backwards and downwards from the pyriform sinus. As soon as the muscle shortens and arches itself the electrode is apt to glide to one side, the position of it must therefore be carefully watched.

The effect of this direct Faradisation of the muscles is to cause the arytænoid cartilages to rotate outwards and backwards, so that the glottis is thrown completely open.

The percutaneous Faradisation of the recurrent nerve is less useful for this purpose, although it is very serviceable in any of

* Die Elektricität in der Medicin. 4. Auflage. Erste Hälfte. 1872. p. 270.

the various phonetic paralyses of the vocal cords. The reason of this is that the glottis does not require opening in such cases. Quite weak percutaneous Faradisation has no effect upon the vocal cords. But a strong current always makes the constrictors contract out of proportion to the others, so that we get a more or less perfect phonetic position of the vocal cords, instead of the wished-for widening of the glottis. To establish this point I made some experiments upon dogs a long time since. I invariably found that when the recurrent was stimulated I got the same position of the cords as is found after irritation of the vagus, *i.e.* the median or phonetic position (the result of the overpowering influence of the constrictors) and not, as one might *a priori* have expected, the cadaverous position. There can be no question that the desired widening of the glottis cannot be brought about in this manner.

We might, perhaps, expect some result from this method of treatment in hysterical paralyses. But we have no trustworthy rules for paralysis having an hysterical basis. A strong physical irritant in such cases is often sufficient to cure the malady at once. Thus sometimes touching with the sound, pencilling the larynx and other irritants are, as in phonetic paralysis, said to be very useful. But personally I have no experience upon this point, for although I have, on the whole, seen a large number of cases of the particular form of paralysis we are considering, I have never yet seen a case of hysterical paralysis of the dilator. And I again warn you not to consider at once that every case of inspiratory dyspnœa with the voice intact is paralysis of the dilator, unless you have some other confirmatory evidence. And special precaution is necessary when the inspiratory difficulty is paroxysmal, with intervals of complete freedom.

When the paralysis is the result of an infectious disease, more especially when it follows diphtheria, the acute exanthemata, erysipelas and the like, the subcutaneous injection of strychnia may be advantageously employed in conjunction with the electric current, and it may be used as it was by Acker * in his experiments upon other kinds of diphtheritic paralyses. Ac-

* Ueber Lähmung nach Diphterie und über subcutane Strychnininjection. Neuro-pathologische und-therapeutische Mittheilungen aus d. Erlanger med. Klinik. Deutsches Archiv f. klin. Med. Bd. xiii.

cording to Acker strychnia may be injected daily for a long time without injurious consequences. I am personally acquainted with a case of diphtheritic paralysis of the dilator in which considerable benefit resulted from the simultaneous employment of electricity and the subcutaneous injection of strychnia, and the improvement was going on the last time I saw the patient.

This mode of treatment by electricity is also useful in those cases where paralysis or paresis of the dilator has followed a long continued inactivity of the glottis, as has several times been seen after the tracheal canula has been worn for a long time.

Breathing compressed air may also be recommended for the relief of the patient's respiratory difficulties, especially in recent cases. But I need scarcely mention that we cannot expect to cure the malady by this means. Also that the general condition of the patient must be borne in mind, and in some cases a strengthening treatment employed, need not be specially dwelt upon.

Finally, we have to consider in what cases and at what period of them tracheotomy is advisable. It is evidently not required in undeveloped cases in which there is merely a slight dyspnœa which only becomes well marked after violent bodily exertion. In those very rare acute cases of the disease there is, on the contrary, nothing else to be done but tracheotomy, and more especially when we cannot anticipate a favourable result from any other mode of treatment. In many cases tracheotomy is imperatively required, because the patient comes under medical treatment at too late a date. Meanwhile the question arises whether it is well to delay tracheotomy until it is vitally necessary, until there is the utmost danger of suffocation, or whether one ought to employ it as a palliative remedy.

As Penzoldt justly remarks, it is usually not advisable to delay the operation until the symptoms of suffocation have rendered its immediate performance absolutely necessary. In well-pronounced severe cases in which, judging from other methods of treatment, it is only a question of a very short time, it is much wiser to operate at once. This course is recommended for this reason, that when there is considerable dyspnœa, the addition of a slight attack of laryngitis, tracheitis, bronchitis, or the like, may bring on violent symptoms of suffocation, of such a kind that the immediate performance of trache-

otomy is necessary to preserve the life of the patient. To avert this danger it is better to perform the tracheotomy previously.

For the reasons before mentioned it is advisable to employ a canula with an aperture at the upper border, so that some portion of the respired air may pass through the larynx. After the performance of tracheotomy, of course the main thing is to strengthen the paralysed muscles lest the mischief become irremovable.

Lastly, I have to mention catheterisation of the larynx, a means only quite lately brought forward by Sidlo * for the relief of this form of disease. In this case the diagnosis was that of paralysis of the dilator, and tracheotomy had been already talked of when Sidlo performed catheterisation. The result was excellent, for the glottis was at once considerably widened and the danger of suffocation removed. The striking effect in this case was explained at the autopsy, for though there was atrophy of the posterior crico-arytænoid muscles, the cause of the paralyses was a cicatrix on the posterior wall of the larynx. It is obvious that under such circumstances only mechanical dilatation could produce any effect. It must be quite otherwise with the particular paralysis of the dilator we are now discussing; mechanical impediments, not falling under the head of paralyses, are naturally excluded in this place. In cases of real paralysis of the dilator catheterisation will no doubt have a momentary effect, but immediately the catheter is removed the vocal cords will revert to their old position. A radical cure by this means is out of the question.

* Über einen seltenen Fall von Glottisstenose. Wiener medic. Wochenschrift 1875. Nos. 26. 27 u. 29.

# THE FOOD OF INFANTS.

BY

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GENTLEMEN,—The question as to what is the best food for infants until dentition takes place, is indeed a very ancient one; but it has lost none of its original freshness for the practising physician, who has to solve it under the changing conditions of individual existence, or for the scientific investigator, to whom, notwithstanding all the progress that has been made, it still presents a rich field for observation.

From a teleological standpoint we must without hesitation assert that until the eruption of the teeth enables the mouth to deal with hard food, the mammary glands ought to do for the child precisely what the uterus does for the ovum.

Theoretically, on *á priori* grounds, every lying-in woman is considered to be bound to suckle her infant, just as, under our present laws, every man that is twenty years old is subject to conscription. But as among the conscripts, some are fit for duty, some only relatively so, others temporarily, and others always unfit, so the like holds good of lying-in women, only with the difference that, in the case of the latter, you can use persuasion both for and against nursing, but have no means of dealing effectually with the large class of deserters. It is certainly true that those whom you counsel not to suckle their children will readily submit to your advice, but the others who could, but will not, do their duty exhibit in this matter that obstinate passive resistance which is so often censured in the tender sex. To these latter you will do no good by merely pointing out the obvious function of the mammary glands. You must bring forward more striking arguments. You may,

in the first place, remind them that suckling a child affords a relative protection against the various diseases of the sexual organs, so prone to remain after delivery. It is a well-known fact that the excitement attendant on suckling induces, by reflex action, those after-pains, which, as they essentially expedite the puerperal resolution of the uterine tissue, prevent, to some extent, the enlargements, the obstructions, and changes of position of the womb, the chronic leucorrhœa, and the menorrhagia, all of which are frequent causes of long-standing hysteria and indeed of secondary sterility. You may also lay stress upon the fact that human milk, because more rapidly digested than cows' or any other milk, wards off many diseases to which the child's digestive organs are prone, excites a more rapid and vigorous growth, and finally, that its use is associated with a far lower rate of mortality among children than is found to attend the employment of all other natural and artificial foods. There are many statistics to this effect, but the following will serve to prove what has just been said. C. Meyer (Journ. f. Kinderkrankh. 1871, ii., 153 *et seq.*) found that of 8,329 children, six months old and under, who died in Munich from 1868 to 1870, 1,231 (*i.e.* 14 per cent.) had been suckled and 7,078 (*i.e.* 84.9 per cent.) brought up by hand. Figures such as these have a more distinct meaning than a host of words.

In the last place you may call attention to the cheapness of the mother's milk, and to the ease with which it is administered, and in cases where, without valid reasons, the mother would like to make over her duties to a wet-nurse, you may allude to the numerous inconveniences, and even dangers, which the engagement of a wet-nurse involves. If you know how to deal with the woman's peculiarities and how to strike the proper chord, if you do not mind the trouble of energetically maintaining your opinion in opposition to her relations, or even friends of her own sex, you will, perhaps, succeed, and then you will generally be able at a subsequent period formally to demonstrate the correctness of your prognosis by pointing to the vigorous condition of the child's development.

Let us now consider the question of suckling as it occurs to us in practice. When should suckling be advised, how facilitated, and when forbidden? A varying picture opens before us.

There is, in the first place, one group consisting principally

of women belonging to the middle and lower classes. Healthy and anxious to suckle, these would consider it to be an offence against duty and custom to give their children anything else but the breast. The healthy development of the little ones, who in the matter of strength and stoutness soon outstrip their less favourably nurtured contemporaries, is an abundant reward for the sacrifice of time and material. In these cases all we have to do, perhaps, is to warn the mothers against continuing lactation for too long a period.

Others, on the contrary, under every variety of pretext, endeavour to avoid suckling their children, perhaps because it is inconvenient to do so, or else because they are afraid of prematurely losing their beauty. Those who dread the labour of attending to children should be reminded that punctuality practised towards the baby from the beginning makes the nursing a very simple matter. On this point very good counsel is given by L. Desser, in his book, entitled "Die Benutzung der ersten Lebenstage des Säuglings zu dessen Eingewöhnung in eine naturgemässe Lebensordnung," Göttingen 3 Auf. 1855. Those women also who fear lest they should prematurely lose their charms may set their hearts at rest. If the appetite continues good, and they are surrounded by favourable conditions in other respects, and if the children do not succeed one another too rapidly, these fears will in all probability not be realised. On the contrary, we often see that women become stouter and more blooming while suckling, and, only at that time, well developed. But even if the cheeks do grow somewhat pale, this is never a sufficient reason for the neglect of such a duty, and it is incumbent upon you, as the future family attendants, to remind those who are negligent, both of their duties and of the consequences of neglect.

A third group would gladly suckle their children were it not that they are too much occupied with the cares of housekeeping and business. Always busy, upstairs and downstairs, from the kitchen to the shop or sitting-room, they can only now and again attend to the child; quiet nursing, at stated times, is impossible, essential as it certainly is. The continued excitement, the various changes in daily work, affect the milk in such a way that the children often become ill, scream, vomit, and suffer from diarrhœa. If you are not able to prevail upon such

a woman to lead a quieter life, or at least to put the child to the breast several times at regular intervals, or if these measures do not succeed, you will often be compelled to stop the suckling, or, guided by former experience, even to forbid it from the commencement.

I have mentioned these three examples in order to show you, that there are not only reasons connected with the state of the body, as you might possibly think, but some of a social character, in addition to others which are brought forward as obstacles to the performance of the duty of suckling children.

Let us now turn our attention to a series of constitutional conditions, and of the more important general and local disorders, in order to see how far these admit of, or prohibit, suckling.

*Age and Constitution.*—Delicate, very young women, as also old decrepid ones in their first confinement, women of a masculine habit (Viragines), those weakened by numerous deliveries and illnesses, by care and hard work, are, as a general rule, but little fitted for suckling. But all the circumstances should be very carefully considered, and, in doubtful cases, the attempt may be made, under due precautions.

*Anæmia.*—This to many physicians is simply a contra-indication, but you should certainly distinguish between the following cases:—In acute anæmia after delivery or of the puerperal state, in convalescence from a slight form of acute disease, the constitution being otherwise good, and then also in that form of anæmia not due to serious constitutional disease but to improper living, you may often recommend that the experiment should be made, with proper precautions, taking care that the woman is well fed but not too abundantly, and allowing her subsequently to take moderate exercise in the open air. In many cases you will find that while suckling is going on, the bodily vigour and weight keep at a satisfactory level, or even decidedly improve. In such, frequently very doubtful cases, our scruples would often be removed or confirmed, as the case might be, could we adopt the practice of regularly weighing the mothers and children. These weighings, however, among private patients, would unfortunately involve too many difficulties and inconveniences to admit of the practice being generally adopted. If, on the other hand, you have to deal with an anæmia which is connected with a severe acute disease,

with what appears to be a retarded convalescence, or with a chronic form due to severe organic disease, serious derangement of digestion, great waste of the humours of the body, then the scanty secretion of milk will naturally prohibit suckling, which, if persevered in, would greatly increase the anæmia, and thus justify the prohibition.

*Phthisis.*—Here you will prohibit suckling, and that not only if the process in the lungs is already physically demonstrable. If the woman springs from a family in which phthisis has occurred, if she is suffering from the prodromal anæmia of many consumptives, if she is of a phthisical habit or has traces of a chronic bronchial catarrh, suckling must be prohibited under all circumstances, for otherwise the loss of milk will essentially promote the development of the disease.

*Scrofulosis.*—Many physicians prohibit suckling for women who show signs of confirmed scrofula, but they do not reflect that by avoiding the Scylla of the mother's milk, they bring the children right into the Charybdis of scrofula. Therefore I ask: Is it proved that the milk of a scrofulous mother makes the child yet more scrofulous than it had already become by inheritance? It seems to me that this question must not be unconditionally answered in the affirmative. Only in cases where anæmia is present in addition to traces of scrofula, when the milk is poor and scanty, and, from former experience, seems, on the whole, less fitted for its purpose than the milk of a wet-nurse, you will put in your veto, and will do so the more readily when the milk of the mother is to be replaced by that of a hired wet-nurse than if artificial food is to be the substitute.

*Venereal Diseases.*—Neither gonorrhœa nor venereal ulcers prohibit suckling. Just as little does syphilis occurring before pregnancy or acquired during the first seven months. You remember the great experiment which was made systematically in the suburb of Paris called Vaugirard, on children hereditarily syphilitic. The majority of those who had taken the milk of their mothers, who had been treated for syphilis, survived; all who were brought up by hand, died. The application of this experiment is obvious. At the same time keep in mind v. Sigmund's advice to commence anti-syphilitic treatment three weeks after delivery. According to the experience of this author, about a three months' treatment is required during lactation.

It is well known that mercury passes into the milk, and, taken in that way, produces less secondary effects upon the child than the administration of calomel or rubbing in mercurial ointment. If, however, you wish to give a syphilitic child to a healthy nurse, point out the danger of infection from sore nipples or otherwise. But even if, as according to the experience of Dr. Günzburg, of the Moscow Foundling Hospital, this danger is only slight, it seems to me very doubtful whether one is justified in exposing for some months, a healthy person, even with her own consent, to the possible infection of so pernicious a disease. If a woman, in whom you find no traces of syphilis, has given birth to a syphilitic child who has inherited his complaint from his father, if you have therefore to deal with that remarkable case where it is assumed that there is immunity from the effect of contagion, or that the disease is latent in the mother, you may permit her to suckle, after pointing out minutely all possible ways in which the disease may be transferred. A woman upon whom the poison from the man has had no effect may certainly not be so easily infected by the child, who, however, will probably die if deprived of the mother's milk. On the other hand, a woman who has become syphilitic during the last two or three months of her pregnancy, should be isolated from the child, if the latter shows no traces of any kind of a specific disease.

*Rickets.*—No doubt you have read in text-books that a rachitic woman ought not to suckle. You are, however, very well aware, that a woman, who has suffered from this affection in her early youth, may afterwards become so perfectly healthy as to be able to fulfil her maternal duties with perfect ease; and you will be able often enough to satisfy yourselves in practice as to the energy with which lactation may go on, in persons who exhibit traces of the rachitic process.

*Osteomalacia.*—In this disease, on the other hand, suckling should decidedly be interdicted, not only because it impairs the general nutrition, but also because the softening of the bones is apt to make very rapid progress, and this, naturally, is very serious, not only at the time, but also as regards future confinements.

*Acute non-contagious febrile diseases.*—Here the determination of the question whether the woman is to suckle or not, depends upon several circumstances; first, upon the constitutional vigour,

as also upon the amount of strength possessed at the time, this latter varying with the kind, stage, and course of the disorder, also upon the amount of milk really secreted, and upon any experience which we may have obtained as to the growth of the child. As no general rules can be laid down, all I can say to you is, allow the woman to suckle if you have reason to hope that after the acute disease has subsided, she will still have enough milk to go on suckling, and will be able to do this without producing exhaustion. The case is otherwise, when the woman is suffering from a disease which we know to be attended with great loss of tissue, and characterised by a tardy convalescence, as, for example, typhoid, severe inflammatory disease, and the like. In such cases, having regard to the possible exhaustion and the tardy recovery, it would not be right to require the woman to go on with her nursing.

*Cholera.*—With reference to this disease, it has been asserted, for example, by Hennig of Leipzig, that children may take the milk from their sick mothers without detriment.

*Dysentery.*—Geach allows mothers who are suffering from dysentery likewise to nurse; it is indeed said that the disease in the mother will then sooner subside.

I will not allow myself to pass any judgment with regard to these two affections, but I have often noticed that the children of mothers suffering from severe intestinal catarrh, also become attacked with diarrhœa, therefore I would rather advise that in all these cases of bowel-affection, the child should be kept away from the mother, until her disease has subsided.

*Acute Infectious Diseases.*—The puerperal processes vary very much in their importance with regard to the child. You know that when a pregnant or parturient woman is attacked with fever, the fœtus may become infected, and may die, with symptoms of septicæmia, peritonitis, inflammation of the umbilical vessels, etc. The new-born child is, of course, also exposed to the danger of putrid infection at the navel, at accidental wounds, excoriations, vesicles of pemphigus, etc.; but whether the milk itself of a woman suffering from severe puerperal disease can infect the child is at least a point still in dispute. In the lying-in wards in Vienna the child is immediately transferred to the Foundling Hospital, if the mother is attacked by any puerperal disease. Others are less scrupulous in this matter, and we

ourselves, as you have witnessed, have allowed the women to go on suckling, if their supply of milk was sufficient, in almost all our cases of puerperal disease, and among these we had serious cases of essential puerperal fever, with secondary local symptoms, and a number of patients with violent endometritis and parametritis. The majority of these children certainly did not increase so rapidly and considerably in weight as those of healthy mothers. With the exception of one case, where the child was attacked with cerebro-spinal meningitis almost at the same time as two others close beside it, the children were healthy when discharged. But in other wards, and repeatedly in private practice, I have seen children die with symptoms of septicæmia or peritonitis whose mothers had been attacked by puerperal fever after delivery, and where, therefore, the fœtus was not infected by the blood of the mother. Cases such as these, and which, moreover, have often been observed by others, should make us careful; and, in my opinion, it is better to take the child away from a woman suffering from a severe form of child-bed fever, to remove it as far as possible from the mother, and to take care that it is not washed and dressed by a midwife or a nurse who has been brought into contact with the lochia or other discharges of the patient.

In the case of small-pox, if a woman suffering from this disease gives birth to a child, the infant should, as a matter of course, be isolated and vaccinated as soon as possible, for, unless vaccinated, it will be extremely likely to take the disease, and you know that small-pox in an infant at the breast is almost certainly fatal. The same holds good of diphtheritis and scarlatina, although infants under six months old are seldom attacked by these diseases. Measles, which, generally speaking, is rare among lying-in women, does not very often attack children at the breast. Among the cases of measles collected by Monti, only 11.2 per cent. occurred in infants. In an epidemic, with ninety-six cases observed by myself, there were only three infants attacked, the youngest being thirty-five weeks old. But if only a few of these children were infected by their mothers, the fact that they were thus infected is a sufficient reason for removing the child from a woman suffering from measles.

*Psychoses.*—A woman liable to hysteria should be forbidden

to suckle only if the disease has become so severe that the slightest impression produces serious general nervous excitement, giving rise to fear that the child may be often unfavourably influenced by the milk. Nursing should also be forbidden when the hysteria is complicated by any other affections.

*Epilepsy, Mania, Imbecility.*—Women with these complaints are not fit to suckle children. They would neglect them, occasionally let them fall, or even attempt to kill them. It has been asserted—but the notion is not even probable—that a predisposition to mental disease may be acquired by the child through the milk. We often see mental disorders transmitted through the mother to the child in cases where the children have not been put to the breast.

Let us, in conclusion, consider certain anomalous states of the mammary glands, with reference to the question whether, and under what circumstances, suckling should be forbidden when these are present.

*Nipples, flat and circumvallate.*—Small, flat nipples, only slightly raised above the level of the areola, cannot at first be taken hold of by the child's lips, or can be only incompletely grasped; but this defect can generally be remedied by regularly drawing out the nipples. This is best done during pregnancy, either with the fingers, or by means of a suction-glass with a small mouth. The same may be said of one variety of the so-called concave or depressed nipples, in which these parts are sunk below the level of the crateriform areola. In a second variety, with a short and tight subcutaneous ring of tissue surrounding the nipple, the latter resists all attempts at extraction. The milk is a sort of dormant fund for the child. In these cases you may render assistance by excising the areola, an operation which I was the first to introduce (v. Berliner Centralbl. f. d. med. Wissensch. 1873, 17). You remove the skin and muscular tissue of the areola until there is only a border left round the nipple, then, by sutures passing from above downwards, you bring together the edges of the wound, and unite them to the base of the nipple now exposed. After the wound has healed, you will have a nipple either flat or moderately projecting, which, in the next lactation, the child will be able to grasp. You will remember that last winter you saw two operations of this kind, in each case on

both sides. One case was that of a pregnant woman. She was operated upon six weeks before her confinement, and was able afterwards to suckle the child at one breast. The other nipple was so short and tightly fixed as not to project sufficiently. The second case, a peasant woman, was non-pregnant at the time, having been confined a few weeks previously. She is now able to nurse her child properly, and with obvious good results, at both breasts. In her former confinement she had tried by every means in her power, but in vain, to draw out the nipples.

*Excoriations and ulcers of the nipple.*—These, as is well known, are produced by friction when the child is sucking. The efforts of the child keep up and aggravate the sores, and suckling thus becomes very painful for the woman. There is, therefore, every reason for stopping the nursing until the sores are healed. This measure, however, is not generally adopted, from the well-grounded fear of the breast becoming dry. The ulcers are cauterised with strong solutions or a stick of nitrate of silver, so as to stimulate the formation of granulations, and cause rapid cicatrisation. Friction is also diminished by applying short india-rubber caps over the nipples. In the great majority of cases the sores heal under this treatment, notwithstanding the continuance of the nursing. But if the ulceration invades deeper parts, or spreads superficially, if suckling causes such violent pain as to disturb the woman very considerably, we must certainly take away the child, at least till the sores are healed. Whether lactation will be subsequently possible depends upon the energy of the secretion.

*Inflammation of the breast.*—Those who consider that inflammation of these glands has galactostasis for its starting-point, allow the child in the first stage to go on sucking. The stoppage of the milk alone appears to me, however, not to lead to mastitis. Just notice whether, among the great majority of women who wean their children at the end of the puerperal period, or after a longer period of lactation, you meet with more than a rare case of painful obstruction. Certainly, at this time, mastitis sometimes occurs, if any other injurious influences have been at work; but you must remember that affections of the breasts occasionally appear when lactation is absent. Moreover, you will remember the experiment which I showed you last summer.

Two teats of a rabbit that was suckling were covered over so thickly with collodion as to prevent the young ones from getting hold of them; the other teats were left free. The glands that were covered were swollen up for some days, but the swelling subsided without any inflammation being produced. They contained thickish milk, the particles of which, having become confluent, were enlarged up to twice or four times the natural size. I must, therefore, agree with those who, like Winckel and others, do not allow that stasis of the milk produces mastitis. This, however, should not prevent us from occasionally squeezing out the glands, in order to diminish the tension, which is so unpleasant for the woman, when there is inflammation of a lobe of the breast, and continuous abundant secretion from the other lobes. This is best done by concentric pressure made by both hands, so applied as to include the whole of the breast. In order to prevent further irritation, we do not allow the suckling to go on; but, if this kind of nursing had been adopted at the commencement, it would, at any rate, be stopped when the stage of suppuration was reached, because the admixture of pus with the milk is generally credited with having at least the effect of disturbing the child's digestion.

*Agalactia and Oligogalactia.*—In the former condition, which is certainly rare, there is scarcely any remedy to be found; for the latter, we may endeavour to improve the constitution, and indirectly the secretion of milk, by the regular administration of good nourishing food, the woman, at the same time, taking exercise about her house, and, when possible, in the open air. In such cases, spirituous liquors, beer especially, are had recourse to, but they are by no means generally advantageous. In addition to these, there are a number of powders and infusions of vegetable remedies, taken from the class of aromatic oils and bitters, which have been recommended for inducing the secretion of milk. Fennel seed enjoys a particular reputation in this respect. If there are any derangements of digestion, you may always make trial of such remedies, but you must not expect too much from them.

*Galactorrhœa*—When this occurs, the woman should be forbidden to continue nursing, so that the anæmia, which is usually present, may not be still further increased.

*Hydrogalactia.*—A condition thus termed from its analogy to hydræmia, and characterised by the secretion of milk containing an unusual quantity of water. It does not necessarily cause atrophy of the child; for if the secretion is at the same time copious, the absolute quantity of solid material, notwithstanding the small percentage, may be enough for all requirements. The excess of water is easily carried off by the kidneys.

Let me, in conclusion, by way of recapitulation, classify lying-in women under four divisions. The good nurses are those who are able at once to suckle their children, and who continue to do so until the complete appearance of the milk incisor teeth. If anæmia, or pains in the breasts and back, should come on while they are suckling, the child should, as a matter of course, be weaned earlier. Those temporarily unfit to suckle may be placed either in the first or in the last group, according to the final result of their complaint. In those that are only relatively fit, all the conditions have to be weighed, in order to determine whether they are to be placed in the first or the last category. Those unfit to suckle owe their disability either to primary or to secondary causes.

A woman is fit to suckle her child when the following conditions are present:—

1. Copious secretion of milk, and good health.
2. Erosions of the nipples.
3. Slight puerperal fevers.
4. Slight diseases in general.
5. The remains of rachitis.
6. Venereal diseases, excepting when the disease has been acquired during the last two or three months of pregnancy.

Temporary unfitness exists under the following circumstances:—

1. Short or concave nipples, until this obstacle is remedied.
2. Deep or extensive ulcers of the nipples.
3. Mastitis (the healthy breast continues to be used).
4. Intestinal catarrh.
5. Dysentery.
6. Cholera.

Relative fitness is due to—

1. Oligogalactia and hydrogalactia.
2. Delicate constitution, decrepitude, debility.
3. Acute, and
4. Simple chronic anæmia.
5. Scrofulosis.
6. Acute, non-infectious febrile diseases of moderate severity.

Unfitness is due to—

1. Agalactia.
2. Galactorrhea.
3. Circumvallate nipples, which cannot be drawn out.
4. Chronic anæmia, the result of severe organic disease.
5. Phthisis, or suspicious prodromal symptoms.
6. Syphilis acquired during the last two or three months of pregnancy.
7. Osteomalacia.
8. Severe puerperal fevers.
9. Acute exanthemata, small-pox, measles, scarlatina, diphtheria.
10. Non-infectious diseases, with high fever and long-protracted convalescence, *e.g.* typhoid, severe pneumonia, articular rheumatism.
11. Violent hysteria.
12. Epilepsy, psychoses.

The question now arises, how are we to feed the children when their mothers do not suckle them?

If the circumstances of a family will permit, whenever the mother's milk cannot be obtained, we have recourse to that of a wet-nurse in preference to any other mode of feeding. I know very well what has been said of wet-nurses before and since the time of Max. Stoltz; but for all that, I can assure you that we cannot do without them, and especially at a time when so many of the female population of our cities devote themselves to intellectual progress at the expense of their bodily health and development.

If you read a treatise on midwifery, or a monograph connected with the subject, you will find that so many excellent qualifications are demanded in a wet-nurse, that it would seem a difficult matter to find a real person corresponding with such a *beau ideal*. The wet-nurse should be gentle and phlegmatic,

clean and respectable; she should be fond of children, not too young, and not too old, and not suckling for the first time, for she then has too little milk. Observe, however, that even under opposite circumstances many suitable individuals are to be met with. Her own confinement should have taken place more than fourteen days previously, for after such an interval puerperal diseases rarely happen, and the lochial discharge becomes slight. Can we in many cases wait for this length of time? If we ask further, with reference to the physiological changes in the milk which occur in the course of time, the age of the nurse's child should not differ from that of the foster-child by more than two months; others say, by more than half a year. There is some truth in this, but there appears to be less disadvantage when the nurse's child is much older than when it is much younger than the foster-child. Good health and an abundant supply of good milk are the most important requirements. From slight superficial examination we cannot get proper information as to the state of the nurse's health. Even the well-made and blooming-looking country girl may have somewhere a well-marked eruption; and, on the other hand, a disagreeable, worn-out looking person may be an excellent nurse. You should, therefore, make a minute examination, and, above all, look out for signs of venereal disease. Other chronic and acute diseases, to which we alluded when discussing the question of a woman's suckling her own child, should also, as a matter of course, prevent a person affected with such diseases from acting as a wet-nurse; indeed, in this respect we ought to be far more scrupulous as to wet-nurses than in the case of mothers.

Syphilis is the principal enemy, and in order to see if it be present, you must make a minute examination from head to foot, and search for any traces, possibly well-nigh obliterated, with the thoroughness of a detective. Hard roundish swellings of the lymphatic glands, spots on the skin, papules, psoriasis palmaris and plantaris, and other syphilitic eruptions, large condylomata about the mouth and genitals, ulcers of the tonsils, hoarseness and difficulty in swallowing—all these have to be especially looked for. Do not be deterred from examining the genital organs of a candidate for the office of wet-nurse. You often find hard or soft ulcers, or pointed condylomata, and

purulent discharge from the vagina, uterus, and urethra, and if the urethra be thus affected, and if you just put the question as to whether there has ever been any swelling of the genital organs, or copious discharge, or scalding pain on micturition, you will find that the answer will confirm your suspicion as to the existence of gonorrhœa. Examination of the genital organs is certainly often neglected, and with impunity; but woe to you if you allow diseases of this type to escape your notice! A possibly life-long illness of the child, a local scandal in addition, which, in such a case, you must confess to have deserved,—these are the miserable consequences which your neglect may possibly cause.

We now pass to the question of the nurse's milk. You examine the breasts with regard to their size (which is by no means always proportionate to the quantity of milk secreted), the quantity and firmness of the glandular tissue, the subcutaneous adipose tissue, the development of which is generally in inverse proportion to that of the proper tissue of the gland. The appearances to be desired are—marked networks of veins over the breasts and walls of the chest, as indicating developed local circulation; radiating striæ of the integument of the breasts, showing vigorous growth during pregnancy; nipples prominent and of moderate size. Recent painful indurations in the glands, or cicatrices of former abscesses with subjacent atrophy of gland-tissue, are very undesirable appearances, and in the latter case, perhaps, there may not be sufficient secreting tissue remaining. You now compress the breasts from the parts around the areola towards the nipple. Plentiful secretion is indicated if, after several hours' rest, the milk spurts out in several streams, or if it trickles out as if from many small sources. If you wish to be very exact, you cause the milk to be removed from both breasts by means of a milk-pump, and then, after two hours' rest, you again collect the secretion which has accumulated. According to Lampériere (Compt. rend. 1850. Vol. 30, p. 173) each breast of a good nurse yields during this period at least from 50 to 60 grammes of milk. Practical women estimate the quantity of milk according as the child, when removed from the breast, shows signs of hunger, or the contrary, and by the wetting of the napkins. To estimate the quality of the milk, it is neither necessary to drop some on the

nail, nor to pour a few drops into a glass of water, and notice the cloudiness, that is, the speed with which the drops divide; these are the old tricks of midwives, by whom they are much thought of. The best plan is to take about a thimbleful of milk in a little glass, and notice the colour. If the milk is white, like cows' milk, it is rich in fat, but if it is of a dull-white colour or bluish, then it is watery. This, after some practice, is a sufficiently approximative test. If you wish to make a more complete examination, you must use the microscope, and see whether there are many moderate-sized and large oleaginous globules, or whether they are more scanty, and, for the most part, small, resembling in size those in cows' milk. In the former case the milk is rich in fat; in the latter, poor in this constituent. As opportunity offers, you may also examine whether the specimen contains any colostrum-corpuscles, or those of pus, or both together, as in mastitis. If these appear in still greater quantity after the eighth day, such milk will, according to Donné, be bad for the child. I will give no decided opinion as to this statement, and will only point out that numerous colostric corpuscles, and horny epithelial cells somewhat less in size, are found in the obstructed secretion of a partially inflamed breast. If you have satisfied yourself by both these methods, that the specimen is poor in fat, and, what is then probable, generally poor in solid constituents, you need not at once pronounce the woman from whom the milk has been obtained to be an unsuitable nurse. If you have a choice, of course milk containing fat is better than that which is poor; but if not, remember that a person with poor milk may make a good nurse if, at the same time, her supply of milk is copious. It is possible that with better feeding the milk may improve, but if not, we know positively from experience that even such individuals often bring up very vigorous children.

Of more importance is the fact that two or three kinds of milk, pretty closely resembling each other in the amount of their fatty constituents, may very differently affect the digestion and nutrition of an individual child. Illustrations of this are seen in children who have had several successive wet-nurses all yielding milk apparently the same as regards the proportion of fat, but who have been very variously affected in

the matters of appetite, evacuations from the bowels, and increase of weight. It is an open question whether these differences may be referred to variations in the quantity of casein, sugar-of-milk, albumen, and salts, or whether, under such circumstances, there may be other variations, of which we are at present quite ignorant, in the composition of the casein, fat, and salts, or some alteration in that of the so-called extractive matters.

I have not as yet alluded to a very important test both of the health of the mother, and of the quality of the milk; I refer to the condition of the wet-nurse's child, and also ultimately to that of her new foster-child. Many important conclusions may be drawn from a careful examination of the wet-nurse's child, in whom also we must especially look for symptoms of syphilis and scrofula. If you find pemphigus on the hands and feet, papules about the anus and genital organs, or upon the lips, fissures at the angles of the mouth, and about the anus, alterations of the skin of the face, particularly spots appearing as though covered with oil on the glabella and eyebrows, marked eczema of the face and other parts of the body, swellings or suppurations of the lymphatic glands—all such symptoms as these should cause us to reject the mother of such a child. The older the child that we are examining, the more valuable will be the information obtained as to any disease of the mother's constitution. Only recently you have seen a case in which the woman became affected with syphilis in the fifth month of her pregnancy. The child was at first vigorous and lively, and throve well, but on the twelfth day we found a small papule on the anus as the first manifestation of syphilis. Cases frequently occur in which it is only after weeks or months that signs of syphilis become visible, whence it follows that the healthy appearance of a very young child is no certain evidence, and does not admit of any conclusion being drawn from it, except when taken in connection with the history and state of the mother's health.

I said just now that the condition of the wet-nurse's child may also be taken as a test of the quality of the milk. That assertion is correct, when the mother's milk has been its only food, and its nursing in other respects has not been neglected. A lively-looking, stout child, with firm muscles and fat, cannot

certainly have been fed on bad milk; but you must not imagine that the foster-child of such a nurse will likewise become strong. The individual peculiarities of children play an important part in this respect, and often upset all prophecies and all calculations, however reasonable they may appear.

With the necessary limitations, and taken in connection with a minute examination of the mother, we may therefore obtain very important indications as to the fitness of a wet-nurse by examining her own child, or a foster-child suckled by her on some previous occasion. And hence the advice: before engaging a wet-nurse, examine her child, if possible, or have it examined by a trustworthy person—one of your own profession, for instance.

We now come to a third class of children—those who can obtain neither their mother's milk nor that of a wet-nurse—the so-called hand-reared children (Päppel-Kinder). These form, unfortunately, a large contingent, of which a vast percentage will die before they are a year old.

Both the wisdom and the foolishness of mankind have exhausted themselves in due form in attempting to devise the most suitable food for children. Although among different nations, and at various times, almost everything that is eatable has been administered to children, the preference has always been given to animals' milk, and things compounded with it, to broth, and articles of food containing starch or sugar.

Let us commence with animals' milk. In accordance with what we shall find to be the case with regard to the reaction of casein, asses' and mares' milk must be regarded as the most rapidly digestible of all the kinds of milk which we here have to consider, and as approximating most closely to human milk. With regard to asses' milk, you are well aware that in England, Holland, and other countries, it is very often used for feeding children; we (in Germany) but rarely have recourse to it, perhaps because this solidungulous tribe is regarded as somewhat of a pariah among our domestic animals, and suckling asses are, generally speaking, difficult to be met with. I am not aware whether mares' milk has been often used for feeding children. I think, however, that experiments with mares' milk, which can easily be obtained in places where horses are bred, are very desirable, and I would venture to recommend a trial of

such milk to those of you who will hereafter practise in such neighbourhoods, and I ask also that you will be good enough to communicate to me the results, in which I shall be much interested.

These two kinds of milk, however, are not available for the majority of the public; we have to consider therefore only cows' and goats' milk.

Here I must, in the first place, correct a very wide-spread view, which is, that human milk differs from that of cows only in the percentage composition, and that we can make cows' milk similar, if not equal, to human milk by adding water and sugar. The calculation has been made thus. According to the mean of Gorup-Besanez's analyses, the two milks are composed as follows:—

| | Human Milk. | Cows' Milk. |
|---|---|---|
| Water . . . . . | 88.908 | 88.705 |
| Casein and Albumen . . | 3.924 | 5.404 |
| Fat . . . . . | 2.666 | 4.305 |
| Sugar-of-milk . . . | 4.364 | 4.037 |
| Salts . . . . . | 0.138 | 0.548 |

From these figures it appears that to make cows' milk resemble human milk as regards protein and fatty constituents, we must add 60 per cent. of water, and then to make it contain an equal amount of sugar, we must add to 160 parts of the mixture 2.4 per cent. of milk sugar. This, however, is an error. The real difference is quite of another kind.

Let us add some vinegar to specimens of human and cows' milk. I select this test, because occasionally it may also be turned to account practically for purposes of differential diagnosis.* This produces no microscopic alteration in the human milk, but in the cow's milk, large firm coagula of casein

* Two years ago I met with a case in which, by using vinegar as a test, we discovered a fraud, the consequences of which had been well-nigh fatal to the child. A suckling woman with plenty of milk had been engaged to nurse with her own a colleague's child, a weakly infant, just recovering from dysentery. All went on well at first. After some time the woman occasionally, instead of presenting herself, sent small bottles containing ostensibly her own milk. The contents of the bottles, however, being tested by vinegar, we discovered that this dearly-bought milk was that of the cow. The child had a narrow escape of falling a sacrifice to this disgraceful fraud.

immediately appear. If you add hydrochloric, sulphuric, or nitric acid, human milk when cold does not coagulate, whereas in cows' milk these acids would produce large flakes and globular masses. Tannin causes coagulation in fine flakes in both kinds of milk. These examples may suffice to show you that the casein of human and of cows' milk is differently affected by re-agents, and by the great majority of acids, alkalies, and salts. This fact has been known for a long time. Simon was the first to draw attention to it, in his excellent inaugural dissertation (De lactis muliebris ratione chemica et physiologica. Berolini 1838; and translated under the title "Die Frauenmilch," etc., Berlin, 1838). It has been asserted that the different tendency to precipitation evinced by the casein of human and cows' milk is not dependent upon a difference in the constitution of the two kinds, but upon differences in the percentage composition of the serum. This view, supported by Scherer (in Wagner's Handwörterbuch d. Phys.), has been hitherto universally current. But it seems to me untenable, in the face of an experiment which I frequently made two years ago.

To make the process intelligible, I must first remark that we collect a filtrate freed from all the solid constituents, according to the following plan, first suggested by Zahn and modified by myself (see my paper on the casein of milk, Archiv f. Gynäk ii. 1 *et seq*). We take a porous cell (such as is used for galvanic purposes), cleansed from all impurity by having been boiled, etc. This is closed by a plug of caoutchouc, into which is inserted a glass tube bent at a right angle. One end of the glass tube reaches into the cavity of the porous cell; to the other is attached, so as to be air-tight, a caoutchouc tube connected with an air-pump. The cell is placed in the liquid to be filtered. By repeatedly pumping out the porous cell the liquid is drawn through its walls into its cavity. The filtrate is entirely free from fat-globules, and from all matters suspended in the liquid. It contains only the salts dissolved in the water, the sugar-of-milk, and the constant, though minute, quantity of alkaline albuminate, which is precipitated by nitric acid. On the other hand, all the casein with the milk-corpuscles is left behind.

If you now, at a low temperature, and as quickly as possible,

in order to avoid decomposition, filter human milk through one cell and cows' milk through another, you will have four fluids—two filtrates, and two remains of filtration containing fat and casein. If now the residuum of the human milk be mixed with the filtrate (serum) of the cow's milk in any quantity you please, and, on the other hand, the residuum of the cow's milk with the filtrate of the human milk, and each mixture treated with various re-agents, you will find that the reaction every time will be that of the residuum—in other words, the filtered residuum of the human milk, which has been mixed with the serum of cows' milk, will not become curdled by acetic, hydrochloric, sulphuric, and other acids; whereas, in the mixture of the residuum of cows' milk with the serum of human milk, the above-mentioned re-agents will cause coagulation.

Now, if the difference in the composition of the serum, the inequality in the amount of the saline constituents, particularly of the soluble phosphates, were the cause of the difference in the behaviour towards re-agents of the casein of human and of cows' milk, the reaction of the residuum of the human milk on the addition of a corresponding quantity of the serum of the cows' milk would so change, that the reaction of the mixture would be that of the milk the serum of which had been added.

But, as we have seen that this is not the case, we are driven to the conclusion that the casein of human milk has a different chemical composition, that its atoms are grouped differently from those of the casein of cows' milk.

This conclusion is still further supported by the behaviour of pure casein of human and of cows' milk towards re-agents. According to the recent observations of Dr. Biedert, the reaction of pure casein is the same as that of the casein when contained in the milk to which it belongs.

I think that I ought to add, that asses' and mares' milk, with regard to coagulation, is affected by re-agents almost in all respects like human milk; but the milk of goats and of sheep resembles cows' milk in this respect.

That, however, which is of most importance practically is the effect of the gastric juice upon human and cows' milk. As it appears from experiments made by Biedert in my presence (see his Unters. über d. chem. Unterschiede d. Menschen-u.

Kuhmilch. Dissert. Giessen 1869), an artificial digestive fluid, prepared from the mucous membrane of the rennet-bag of the calf and dilute hydrochloric acid, at first precipitates the ca sein of human milk; but the fine coagula are again dissolved on the addition of an equal or larger quantity of the fluid. The casein of cows' milk is thrown down in globular masses, but these are not dissolved upon the addition of more fluid. If we put human and cows' milk into two glasses, and add a small quantity of artificial gastric juice, and place them in a water bath or oven at a temperature of 38°. C., the coagula of the human milk will be very much more rapidly digested than those of the cow's milk.

It follows from this demonstration, that it is by no means sufficient to add water and milk-sugar to cows' milk, until we obtain similar percentages of the principal constituents. Such a fluid is far from being analogous to human milk, for which it is at best a very defective substitute. To use a very ordinary comparison, it bears the same relation to human milk as an infusion of chicory or figs does to that of real coffee.

Let us estimate this knowlege at its full value, and draw a few practical conclusions therefrom. In the first place, we should use every effort to induce a negligent woman to nurse her own child; and, in doubtful cases, having regard to the child's welfare, we should express ourselves rather for than against the suckling, at least whenever artificial feeding is the only alternative. If, however, it is impossible for the woman to suckle, and we can choose between engaging a suitable wet-nurse and bringing the child up by hand, a due consideration of the chemical differences between the two kinds of milk would cause us to give a decided preference to the former method.

The above-mentioned facts appear also to explain the differences in the condition of children brought up on human milk and on cows' milk. It is only a minority of the children who are reared with cows' milk that thrive well or even tolerably. This minority consists of those children who have good constitutions, and are well attended to. If we watch them more closely, we shall find that even the healthiest are behindhand in their growth (Gregory), and that their general development is much less than that of children brought up at the breast. At the period of dentition they are generally more

flabby, paler, and more weakly than those children who have been nursed. Under the most favourable circumstances the fæces are yellow and soft, but they are frequently dry and pale, and then, on account of their knotty hardness, their discharge requires great efforts, and is often accompanied by colicky pain. Their pale colour depends upon the abundance of undigested casein and fat. To this richness in casein is due their insolubility in boiling solution of soda, in which the normal fæces of children so far dissolve as to leave only small buttons. At frequent intervals, especially at the period of dentition, diarrhœa is prone to attack even the healthiest of the children fed on cows' milk. You see, Gentlemen, that this minority, which has been usually brought forward as demonstrating the excellence of cows' milk as a food for children, is, when closely regarded, by no means so favourably situated. The majority, consisting of those primarily healthy, but fed irregularly or not with the best cows' milk, or not kept clean, or the progeny of diseased parents, especially scrofulous children, exhibit, particularly during the first year, so many kinds of disorders of nutrition, that a long time is required for the consequences to disappear, even if the children survive; but a proportion of them, varying from one-third to two-thirds, die during this period. The children may possibly continue well for some weeks. They may drink, and with eagerness, an undue amount of milk; but, when closely examined, they are found gradually to diminish in weight, and to become pale and flabby. They constantly cry, they look languid and feeble, and are less energetic in their movements. These changes are accompanied by vomiting and diarrhœic discharges of various kinds with great meteorism. At last a regular inanition fever becomes developed, to which the children rapidly succumb, unless it be checked by proper food and treatment. These are the cases in which telegrams, asking for a good wet-nurse to be sent immediately, are despatched in hot haste to the nearest lying-in hospital. If a good wet-nurse is forthcoming, or, in case of need, a provisional substitute, who will suckle the little patient as well as her own child, the former will then soon become more lively, and move itself more vigorously; its dull, languid look will become clearer and more cheerful, and it will soon begin to

smile again. The meteorism will disappear, the abdomen become softer, the stools yellow and pultaceous, and the loose, flaccid skin will have its adipose tissue restored. A few weeks may suffice to convert the skinny skeleton into a plump, blooming child. Besides human milk, we may have recourse to an exclusive gruel diet, or to Nestlé's food for children, and we shall find that these or any other proper changes of diet will produce an improvement. But, if we go on as before, and make no change, the little patients will suddenly refuse to take food, and then rapidly succumb. We shall find, on post-mortem examination, in addition to great emaciation, gastric and intestinal catarrh, dysentery, follicular ulceration, etc., and the intestines either relatively empty, or distended with gas, and abundant remains of undigested milk.

You will notice, in conclusion, that many children suffer from rickets, as a consequence of being brought up on cows' milk, or else that their general development is so inadequate, that, unless at the period of puberty, when their growth receives a second impetus, they are placed under favourable external conditions, they will always remain stunted and delicate, and of weak bodily conformation. It is on this account that, for us as obstetricians, these children brought up on cows' milk are particularly important objects, for they furnish the majority of instances of flat, rachitic, and generally narrow pelves.

There are a few bright points, as well as these many shadows, connected with this feeding of children with cows' milk, and I will mention the most prominent. Children who have safely passed such an ordeal often, as time goes on, adapt themselves better to their food than those who had formerly been nursed at the breast. Their intestinal canals have had, if I may be allowed the expression, more practice in the digestion of difficult substances, and a change of diet is more easily managed by them than by those accustomed only to the easily-digested human milk.

If you wish, or if you are compelled, to bring up children on cows' or goats' milk, and it comes pretty much to the same thing which of the two you select, you must at least take all care that the following conditions are fulfilled. The milk should come from one cow. If you have a new-born infant to

deal with, choose a cow that has recently calved. The animal should be fed on hay or aftermath, and by way of liquid food it should have mashes of bran, grain, oil-cake, potatoes; in a word, it should be fed on so-called dry food. Grass, clover, swede turnips, beetroot, and such like green food should all be avoided; for the milk, under such feeding, often disagrees with the child and makes it scream, and produces green, or even diarrhœic, motions. The milk should be as fresh as possible when given to the child; circumstances permitting, it should be taken from the cow two or three times a day. If the milk can be obtained fresh only once a day, you should recommend that it be boiled immediately, and a little carbonate of soda added to it, in order to check fermentation. Generally speaking, I would advise the use of boiled milk; for experiments have proved the subsidence of diarrhœa when the milk has been thus treated and the child's diet so far changed. Pure cows' milk is never well borne by children, and we must, therefore, always dilute it. At first, we should give boiled milk, mixed with boiled water in the proportion of one part to three; later on, in equal parts; and milk without water, not until the fifth or sixth month. In addition to this, we should add about 30 grammes of sugar-of-milk to each litre of the mixture. This milk is placed in a small glass bottle, with an india-rubber nipple, and is heated in warm water to such a degree that the bottle may be applied to our sensitive eyelids without causing any unpleasant sensation. After every time that the bottle is used, it must be thoroughly cleansed, and the india-rubber nipple placed in fresh water. We do this to prevent any adherent particles of milk from turning sour. In the last place, the milk should be given not more frequently than every two or three hours; generally speaking, not so often as that; and then only if the child shows signs of hunger by screaming, sucking its hands, or an outstretched finger. Feeding it more frequently has been observed to produce dyspepsia, and even gastro-intestinal catarrh, both of which affections disappeared when the food was given at longer intervals. In addition to this, the observance of punctuality in this case also will considerably save the nurse's time—another reason for adopting the plan of feeding at long intervals. How it is that more frequent feeding is so injurious, I cannot exactly explain.

It may be that the hyperæmia of the gastric mucous membrane, which follows every introduction of nourishment, leads at last to gastric catarrh when the process is too frequently repeated.

The quantity of the cow's milk to be provided has been reckoned by Bouchut (*Journal Official*, 1870, 23 Nov.) in the following manner:

1st month, 10 meals per diem of 4 tablespoonfuls of cows' milk.
2nd „ 7 „ „ 6 „ „
3rd „ 7 „ „ 8 „ „
4th *et seq.* „ 7 „ „ 10 „ „

The above quantities are irrespective of the necessary dilution with water.

If you follow out these rules, and cause the child to be every day carefully washed for five minutes in a bathful of water, at a temperature of 28°. R., and well rubbed afterwards, if you have it kept clean in other respects, and sufficiently warm, you may possibly obtain good results with this kind of feeding. I know a family of eight blooming daughters, all brought up on cows' milk by a conscientious nurse, who strictly carried out the above rules.

You must, however, and I lay great stress upon this, strictly insist upon the daily use of a warm bath while the child is being fed upon cows' milk. The bath exercises a decided influence upon digestion and nutrition, and I could lay before you a great number of examples of children brought up by hand, who had become much reduced, and in whom simple baths, and possibly the daily exhibition of a few teaspoonfuls of Tokay, Malaga, or other wine, produced a remarkably rapid improvement and increase of development. We may fairly suppose that the warm bath actively stimulates the absorption of chyle, and the peristaltic action of the bowels.

But to all this you will reply, these are doubtless very good rules, but they can but seldom be carried out in practice. That I confess; but at the same time I remind you that the non-observance, or the impossibility of carrying out these rules has cost the lives of thousands of children. But be certain on this point, that as the family physician you are to some extent a law-giver. If you give the best possible directions, such as most closely approach your ideal, you need not feel yourselves responsible for the defective manner in which they are carried out.

It is, however, nowadays very difficult in towns to get cows' milk of good and uniform quality; adulterations also are very common, and, therefore, of late years, the condensed milk (Extractum lactis), which is prepared on a large scale by the Anglo-Swiss Condensed Milk Company at Champery, at Sassin, in Hungary, and at Vivis and Kempten by the German-Swiss Milk Export Company, has come into pretty general use. This extract is made by inspissating cows' milk at a low temperature *in vacuo*, by means of steam-heated apparatus; it is then mixed with cane-sugar to preserve it. According to the analysis of Werner, of Breslau, and Kofler (Wittstein's Vierteljahrsschr. für Pharm. 19, 207) it consists of water 18.8—22.4; fats, 12—13.6; sugar-of-milk, 14—18; salts, 2.1—2.6; in addition to insoluble salts, according to Werner, 1.73; casein and albumen, 24.2—28.1; cane-sugar, 24—30 per cent. Before using, four or five times as much water should be added to the semi-fluid mass. You see from this analysis that the condensed milk contains 38—48 per cent. of sugar, and that, even when diluted with five times the quantity of water, there is about twice as much sugar as in human milk. This is a disadvantage for the child, because the sugar becomes converted in the intestines into lactic acid, which is well known to act as an irritant to those parts.

Besides the freedom from adulteration, the general uniformity in the composition of the condensed milk is certainly a recommendation; it is, therefore, particularly convenient for dwellers in towns. Where fresh cows' milk can be obtained, as in the country, its use is to be preferred to that of the condensed milk. Speaking generally, I cannot unconditionally join in the eulogies which many of the laity, and many physicians, have bestowed on the condensed milk. Though children of good constitution may finally thrive, even if fed with this preparation, more delicate, and especially scrofulous, children, thus brought up, suffer just as usual, if not more frequently, from various disorders of digestion. Then you must add gruel or barley-water, and plenty of antacid substances, on account of the acidity of the intestinal secretions caused by the excess of sugar. Generally speaking, in children fed in this way, we find, as Daly has pointed out (*The Lancet*, 1872, ii. No. 18,653), all the disadvantages of artificial feeding very prominently displayed.

Besides diluting the cows' milk with water, other substances have often been used for this purpose, such as fennel tea, anthyllis tea (Hanner), oatmeal gruel and arrowroot (C. Meyer); the intention being *not merely* to diminish the quantity of the casein taken, but also to prevent colic, and gastric and intestinal catarrh. Others—J. Vogel, for instance—have added bicarbonate of soda, with the intention of making the casein more easily digestible. Dyes recommended the addition of chloride of sodium to skim milk, apparently for the purpose of stimulating the gastric digestion. It can be sufficiently proved by experiment that these additions of alkalies do not increase the rapidity with which the casein is digested, and they are positively advantageous in so far only as they act slightly on the digestive mucous membrane.

The casein of cows' milk is difficult of digestion, and in order to impose smaller quantities on the child's intestines, a mixture has been suggested of sweet cream and water (Ritter von Rittershain), or sweet cream and whey. This I have recommended, on fitting occasions, in cases where the necessary quantities of good fresh cream and whey could be procured. Dr. Biedert has, very recently, warmly recommended a "cream mixture," consisting at first of .125 lit. of sweet cream, .375 lit. of boiled water, and 15 grammes of sugar-of-milk; subsequently the proportions to be .5 lit. of cream, .25 of water, to 10 grammes of sugar. He states that this mixture is particularly useful for cases of dyspepsia and intestinal catarrh. It contains, in the first instance, only 1 per cent. of casein.

Amidst the diversities of private practice it cannot easily be decided to what extent one or other of these additions is preferable to the rest; to settle that question, we require systematic experiments carried out upon a large scale, and under external conditions as nearly alike as possible.

In conclusion, I will mention just three of the many substitutes which have been much employed of late years. These are Liebig's soup, Löflund's food, and Nestlé's farina for children.

In bringing forward his soup for children (Suppe für Säuglinge von J. V. Liebig. Braunschweig. Vieweg. 1866. 2. Afl.) Liebig has intended to replace the blood- and heat-forming materials contained in milk by wheaten meal, which has been converted by fermentation into dextrine and sugar. For this

purpose, one part (half-an-ounce or a tablespoonful) of wheaten meal is well beaten up with thirty parts of cows' milk and heated. Then one part of malt meal, with about a thirtieth part of carbonate of soda (to neutralise the acids in the meal) is mixed with two parts of water, and added to the above mixture. After standing for half-an-hour in warm water (not exceeding 66° C.) it is then heated to boiling point, and passed through a hair-sieve. The difficulty of the preparation is that the malt has to act on the wheaten meal at a high temperature, but not exceeding 66° C. Higher degrees of heat destroy the diastase, and produce a pulp instead of a fluid soup, and this, as a matter of course, possesses all the disadvantages of the merely soaked but unchanged starch-flour. On account of the excessive minuteness required in its preparation, Liebig's soup cannot be recommended for general practice. Opinions are now less sanguine than at first as to its value. Children from two to three months old tolerate it better than new-born infants; many children take it freely and thrive upon it better than upon other substitutes. But upon the whole, it is no better than diluted cows' milk. According to Poppel's extensive observations (Berliner klin. Wochenschrift 1869. No. 46) there died—

| | Diseases of organs of digestion. | Other diseases. |
|---|---|---|
| Of children fed at the breast | .. .9 p.c. | 6.7 p.c. |
| „ „ on cows' milk | .. 21.2 „ | 3.5 „ |
| „ „ Liebig's soup | .. 20.0 „ | |

The so-called Löflund's food for children is, in all essential respects, Liebig's soup, but made up into a convenient condensed form as an extract. It is dissolved for use in warm milk, and is particularly recommended for weakly and rachitic children.

Liebig's conception has finally been realised by Nestlé in another way and after another plan, in which the difficulty of preparation is thrown upon the chemist; a very little further preparation makes it fit for use. With superheated steam and under a pressure of a hundred atmospheres, Nestlé (Ueber d. Ernährung d. Kinder v. H. Nestlé, Chemiker, Vevey 1869) converts the wheaten-starch into dextrine and sugar, though only partially, for his preparation, when boiled in water, is coloured dark blue by tincture of iodine. The gluten is converted by this operation into a soluble form. A sufficient

quantity of salt and milk is added, and the result is a fine yellowish powder with a sweetish taste resembling rusks. Jacobsen pronounces the Nestlé meal to be a pulverised compound of wheaten-meal, yolk of egg, condensed milk, and sugar (Industrieblätter 9. 1872). According to Manod (France médicale. 1869. No. 41) this meal (Farine lactée) contains in 1,000 parts 19.50 of nitrogenous substances, and 7 parts of salts. Jacobsen (*l. c.*) found it to contain 40 per cent. of cane and milk sugar, 5 per cent. of fat, 15 per cent. of protein substances, 30 per cent. of dextrine and starch. The powder is boiled with 6—10 parts of water, according as we wish the mixture to be of a pultaceous or fluid consistence. Morpain (France médicale, 1870. No. 19) made the first extensive series of experiments with this food; he tried it on 100 poor children. The mortality, which among such children had previously been from 50 to 60 per cent., was reduced to 20 per cent. under the use of this food. From the experiences of this preparation in various parts of the world, it may be asserted that it deserves to occupy a very prominent place among the substitutes for the natural food. During its use, the development of the children certainly progresses; the food also very rarely causes any disorders of digestion, but on the contrary is easily digested, and is likewise a remedy for any previously existing intestinal catarrh. I may add that children generally take it freely because of its pleasant taste.

I can recommend to you this preparation as the next best thing to human milk, and especially for delicate children, with regard to whom you particularly have reason to fear that they will not bear any substitute for their natural food. As a matter of course, you must not look upon this preparation as a panacea. Even under its use, you will find that children will suffer from general atrophy, rachitis, and so forth, but these affections will occur more rarely than when other artificial foods are employed. Human milk is, and ever will be, the best food, and the one for which no real substitute can be found.

To complete our account of these substances, we must add that meat-broth with or without the addition of mucilage or yolk of egg, used exclusively for some time and afterwards alternately with milk, often proves to be of great service for children who have become much reduced in consequence of bad feeding; it has indeed saved life in such cases.

One remark, in conclusion, with regard to the food of children in general. We know very well that certain articles of food have very diverse effects upon different adults, both as regards the nourishment obtained and the action upon the intestinal canal and body generally. This holds good in a more especial degree with regard to children whose intestines are excessively sensitive to slight variations in the composition of food. The milk which agrees well with one child may disturb the digestive organs of a second or third. Even its mother's milk may sometimes disagree with the child who may be well able to take the milk of another woman. Instances have been reported in which sickly women have reared none of the children they themselves have suckled, while their other children brought up by a wet-nurse or on cows' milk, have survived. Individual peculiarities here play an important part, and one, moreover, upon which we cannot calculate beforehand. The lessons to be drawn from this are, not to adhere too firmly to any one article of food in any given case, but to be guided always by the result, and to keep in view several varieties of food, so as always to have some to fall back upon, when one or another has been found unsuitable.

# SYPHILITIC DISEASES OF THE BRAIN AND SPINAL CORD.

BY

PROF. WUNDERLICH,

LEIPZIG.

GENTLEMEN,—For several years past we have had in the wards a young woman who has presented, and still presents, very peculiar symptoms. It is only a few days since that she was able to move her legs for the first time after a paralysis lasting four years. She is fairly well nourished, and has a pretty good colour, does not give one the impression that she is ill, her features are not unintelligent, though somewhat flaccid, and she moves her arms without any difficulty. But the legs are wasted and weak, though she can move them about in any direction. When questioned, she answers intelligently; but there is this defect, she adds "n" or "ern" to most of her words, and frequently begins with "isn;" she also divides her sentence in a peculiar way, makes use of the infinitive of the verb like a child, and arranges her words wrong, as, for example, "ichn nichen wissen." In reply to the question, "How are you?" she says, "Isn gut gehen." Moreover the speech is slow and stammering. She often stops for a long time in the middle of a sentence, and can get no further. When asked to count, she does so, but begins with "isn," and adds "en" to each number, and omits the "3." This she now frequently describes, though formerly she left it out altogether. She counts thus—isn einsen, isn zweien, isn zweien und einsen, isn vieren, isn fünfen, isn sechsen, etc. Farther on she leaves

out "13" and "23," as well as all the numbers from 30 to 39 inclusive, or else she adds 20 and 10 and the small numbers together.

Only a short time since she left out the "7" and "70," as well as the "3." When these numbers are said to her, she cannot repeat them, neither can she write them, although the others present no difficulty to her. It was further discovered in conversation with her that she was unable to mention the names of some familiar objects, *e.g.* a spoon. At an earlier period she could not find the word for a fork (Gabel). She could repeat after one the syllables "ga" and "bel," but not the two joined together. A short time ago she suddenly found the entire word; but "Löffel" (spoon) she is still unable to say. If the object is shown her, and she is asked to name it, she says, "ichn nichten können sagen," or "nu isn Ding zum Essen." If a spoon be shown to her, and she be asked, "Is it a knife?" she will shake her head; "Is it a fork?" and we get repeated denials, with signs of vexation, and sometimes with tears. If she is then asked, "Perhaps it is a spoon?" she is extremely delighted, and nods her head. She cannot say "coffee," but describes it as "black and white in the cup." Neither can she say the name of her brother Max, though she recognises it amidst other names mentioned to her; and even then, if she is asked, "What is his name?" she cannot tell you.

This condition is the residuum of an extremely severe malady.

The girl got the disease in her seventeenth year, but only exhibited slight marks of general infection. About a year afterwards, now five years since, she was seized with violent headaches and attacks of giddiness; a few weeks after this she fell into a deep swoon, which lasted half an hour. Fourteen days subsequently most violent convulsions came on repeatedly, and lasted for half an hour, accompanied by loss of consciousness, whilst between the attacks the sensorium was not unaffected, to say the least. This state of things lasted for several weeks, the fits recurring from three to five times a day. At the same time a rapidly increasing amblyopia made its appearance; and with this there was much palpitation, oppression at the chest, and total loss of appetite. She came into the

hospital in January, 1870; the body was well nourished, and the colour of the face normal. The tonsils were slightly enlarged, and at the elbows and neck there were some moderately swollen, painless glands; there was no motor paralysis. In the early period of her admission she still had numerous irregular convulsions, sometimes followed by intermittent delirium, sometimes followed by deep coma, lasting for more than an hour, together with violent transitory pains, a moderately frequent pulse, and normal temperature. Under a methodical inunction of mercury, which was begun on the 25th of January, the convulsions became less frequent, the consciousness clearer, and the headache less severe; on the other hand, paresis of the right facial nerve and anæsthesia of the trigeminus of the same side made their appearance, the strength of the arms and legs diminished, and the skin covering them became insensible. On the 21st of February a maculous exanthem broke out on the forehead, and the latter became covered with thin scales, which spread slowly over the breast and shoulders. The patient gradually lost interest in everything, whilst the convulsions ceased. The mercurial treatment was suspended. When she began to speak again in March she behaved like a child, used the infinitive of the verb, and could neither stand nor sit; she had completely lost the sight of the right eye, and was amblyopic with the left, although nothing remarkable was found when the eyes were examined with the ophthalmoscope by a specialist. She now began to omit some of the syllables in the words she used; many words she had completely lost. If she could repeat a word, she forgot it again immediately; sometimes she could not say it at all. So in writing; some words she could not write at all, others only when they were shown to her. Her articulation was imperfect and stammering. Her spirits were good, even extravagantly so, though sometimes there were hallucinations, and a tendency to weep. Under the influence of iodide of potassium, baths, and laxatives, her condition (including the blindness) improved; at first it fluctuated, but since then the improvement has been steady up to the present moment.

The second patient is a young tradesman, and has only been in the hospital some few weeks. He owns having had a chancre some years since, which was followed by an eruption two years

ago. Since that time, but previous to his present illness, he has had mischief in his chest, has once spat blood, and was obliged to remain at home. Some time since he noticed a difficulty in walking, which increased so much that he could only walk a short distance on level ground without a stick. About six weeks after the mischief began, he came into the hospital; he was then well nourished, non-febrile, and the intellect was clear. There was some slight indolent enlargement of the lymphatic glands in the neck. The patient was hoarse, and the larynx was red and swelled from the epiglottis down to the vocal cords, but there was no ulceration. The head, the upper extremities, and the trunk were moved without difficulty, but the movements of the legs, general and co-ordinative, were much impeded. Still he did not drag the legs, like a person suffering from tabes. Sensibility was everywhere normal. On the fourth day of his residence in the hospital, having up to that time felt quite well, he was suddenly taken ill, and noticed that he could not see. Both pupils were widely dilated and fixed. The ophthalmoscopic examination of the eyes, which were totally blind, was very difficult, owing to their constant movement; nevertheless, we proved that there was neither extravasation nor retinitis. The urine was quite normal. On the other hand, the temperature was somewhat raised and the pulse quickened, and there was evidently some mental confusion. The treatment by inunction was commenced.

On the second morning there was complete coma, in which the patient sometimes uttered an inarticulate cry. On the morning following there was remarkably strong general hyperæsthesia of the skin, with absolute loss of consciousness. When the skin was touched in the lightest manner possible the face became disturbed, and the patient shrank away with groans. Leeches were applied behind the ears, and an ice-bag was applied. The temperature up to this time was normal, and the pulse quiet. The reflex movements ceased in the afternoon, and the patient raised himself when asked to do so; but he could neither speak nor see. At 8 a.m. the next day he suddenly cried out, "I can see again now," but fell back again insensible. He began speaking again in the afternoon, although there was partial aphasia, and he could see well enough to grasp a hand held before him. The improvement continued

on the following days. But on the ninth of his admission, hallucinations and delirium set in suddenly. For a short time they kept within bounds, but on the night of the twelfth day they became furious, although they yielded to morphia. Owing to the gums becoming affected, the mercurial inunction had to be discontinued, and iodide of potassium was given instead. After this the condition rapidly improved in every way, the psychical symptoms first. Only five days afterwards, reading and writing could be performed, and now, fourteen days after the last maniacal attack, he can, as you see, walk without support, though it is with some difficulty, and there is no trouble with the eyes. He is going on with the iodide.

In the third patient you will at once recognise the choreic movements in the face and upper extremities; but there is also a restlessness of the muscles of the trunk and legs. When the patient thinks he is quite unnoticed and does not attempt any movement, the muscles are fairly quiet. As soon as he sees that he is an object of observation oscillatory movements begin, and gradually increase in intensity. When he is asked to do anything we get a mixture of agitation, jerking, and rotation, which make the accomplishment of the desired movement a matter of difficulty, and the attempt abortive.

Nevertheless his present condition is only a shadow of that which he has exhibited for months past.

The patient, a strong labourer, twenty-eight years of age, came into the hospital six months ago suffering from most acute mania. It was said to have broken out a few days before his admission, and was preceded by convulsive movements of the arms. When he first came in he was in the wildest excitement, threw his arms about, shrieked, and could scarcely be restrained. The excitement soon moderated. The patient lay quietly in bed for some days. All at once one night he jumped out and clambered up on to the joists of the ward, and, when he was spoken to, thought the roof was on fire. He was got down by means of a ladder, and lay down quietly in bed as if nothing had happened. But the maniacal excitement returned on the following nights. Having discovered that four years previously he had suffered from a chancre, and that the lymphatic glands in the neck, elbow, and groin were hard, indolent, and much enlarged, we ordered him injections of corrosive sublimate and

iodine to be painted on the head. Three weeks after the commencement of the disease, and after the seventh injection, a scattered pustular eruption, forming thick crusts, made its appearance upon the back of the head and over the legs. The maniacal paroxysms recurred almost every day, and the movements in the intervals began to get choreic. He was very delirious, and there was now and then aphasia, for he was unable to find words and expressions for his ideas. When he attempted to walk he tumbled about like a drunken man. Nevertheless there was a gradual improvement at the sensorium, whilst the choreic movements became stronger, and continued to increase in intensity even after the senses were completely recovered, and made the whole body tremble, and prevented the patient taking any rest. In the meantime, owing to the gums becoming affected, the injections of corrosive sublimate were exchanged for iodide of potassium taken internally. The choreic movements of the muscles have gone on slowly improving up to the present very supportable state, whilst the psychical difficulties have completely disappeared, only a certain slowness in the flow of ideas, with hesitation in the speech and a strange embarrassment remaining to remind one that they ever existed.

These three cases, though so different in their symptoms, are in reality identical; they are cases of syphilitic disease of the brain.

Syphilitic diseases of the nervous centres are eminently important, especially from a practical point of view.

There is no disease of the internal organs of which the successful treatment so imperatively demands a correct diagnosis, and where, without this, all means of cure are so abortive and useless, as the syphilitic diseases of the brain and spinal cords, nor is there any other where the alleviation of the suffering and even the life of the patient so undoubtedly depend upon the insight of the physician. How often have we to confess to ourselves with shame that the thanks of the patient have been earned by very questionable services! How often that nature has done the chief part in the most beautiful cures! But the physician has really earned his thanks when he is successful in removing a syphilitic affection of the brain. If the constitu-

tional causes be overlooked in a disease of the brain depending upon syphilis, all curative measures will be futile. If left alone, or useless remedies be employed, the patient suffers the most excruciating agonies, or decays visibly both mentally and bodily day by day. Very often from the moment that he comes into the hands of a physician who recognises and treats the malady, the progress of the disease is stopped, and in a surprisingly short time the improvement is apparent even to outsiders. In such cases as this, by a correct diagnosis we can save the patient from unutterable misery and gain honour and respect for ourselves; there is many a man who has gained a reputation and practice by the recognition and cure of a case of cerebral syphilis undiagnosed by his colleagues.

Diseases of the brain, the result of syphilis, frequently occur; I have seen some hundreds of cases of this kind; syphilitic diseases of the spinal cord are, to say the least, not uncommon. Nevertheless these affections are often mistaken or wrongly interpreted. It is quite a common thing to meet with patients suffering from mischief in the brain whose numerous medical advisers have not had the most remote idea that the source of the trouble was syphilis, and who have been vainly treated in the most various ways for a long time, but who quickly recover, or are, at all events, very much improved by an appropriate treatment.

Syphilitic affections of the brain are but little mentioned in the literature of syphilis; indeed, cases of this kind do not usually fall into the hands of the specialist in syphilis, they are met with much more commonly by the general physician, or by the specialist for the eye, the ear, the brain, or women's diseases. In the general works on medicine these maladies are either omitted altogether or they are mentioned in a very incomplete and unsatisfactory manner. And yet attention has been frequently and forcibly drawn to these practically important changes. I may mention Prof. Jaksch's excellent lectures upon the theory of syphilis in the internal organs (Klinischer Vorträge über die Lehre von der syphilis innerer Organe), which were unfortunately published in a but little known journal (Prager medicinische Wochenscrift 1864. Nos. 1—52), and Griesinger's observations in the Archives of Therapeutics (Archiv der Heilkunde 1860, p. 68—81). And my friend, and

for many years my faithful colleague, Prof. Heubner, has quite lately published an excellent monograph (die luetische Erkrankgung der Hirnarterien 1874) upon the subject, founded upon cases which he has seen in my clinique; by means of careful investigations he has clearly laid down the anatomical character of many syphilitic diseases of the brain, and has been the first to point out the very important nature of the changes going on in the cerebral vessels; these changes had been but little noticed previously. These alterations of the arteries are of themselves sufficient to account for the severest attacks, not only in those cases where the brain is apparently sound, but also in those cases where it is evidently unsound, without corresponding symptoms.

Owing to syphilitic diseases of the brain being so amenable to treatment, it is no doubt very often impossible to verify their anatomical character. Still the similarity between the cases which are fatal and the cases which are cured, justifies us in concluding that they are anatomically similar, with this reservation, however, that many of the anatomical changes do not admit of comparison; thus in cases which get well those changes only could have occurred which either admitted of repair or were without influence functionally. Therefore in the account of the autopsies of those persons who recovered from this disease we could not expect to find that more than certain definite forms of mischief were met with.

Very varied changes in very various parts are met with in the bodies of persons dying of syphilitic disease of the brain and spinal cord. Bones, skin, nervous substance may be affected at almost any part. We frequently find multiple changes. Some are sequelæ and are similar to non-syphilitic products, such as hæmorrhages, inflammations, and disturbances of nutrition; others are more peculiar to syphilis, especially the gummatous tumours. But there are many which are altogether peculiar, and can, therefore, throw no light upon the cases which recover. Moreover in a large number of fatal cases it is impossible, or extremely difficult, during life to explain the symptoms from the foregoing anatomical changes. In addition to this many a well-marked alteration, owing to its occupying an insignificant position, has evidently had no influence upon the symptoms, and, on the other hand, many cases characterised

by well-marked symptoms have presented no great anomalies of structure on an examination of the body after death.

If, in addition to all this, we bear in mind that a rapid improvement or recovery from very severe attacks is incompatible with the existence of any considerable anatomical change, we must admit that the pathological anatomy of cerebral syphilis, as it was formerly understood, is of but little value as far as diagnosis is concerned.

In this state of things, the changes in the cerebral arteries, which Heubner was the first to study accurately and place in their true light, and which previously had been so little noticed, are of the utmost importance.

These alterations in the thickness of the arterial walls, and in the calibre of the canals, which are easily and usually overlooked on dissection, quite explain how it is that sometimes after the severest symptoms we get negative results at the autopsy. There is no doubt that the disturbances of the circulation which these changes in the arterial vessels bring about are often the cause of the symptoms; whilst a very manifest growth, in the immediate neighbourhood it may be, may, owing to its position, have no effect at all, or only a subordinate one, on the symptoms. Owing to the fact that, from these changes, difficulties may arise at any point in the cerebral circulation, we have an explanation of the polymorphous, widespread, and changeable character of the symptoms. They explain how it is that severe symptoms may suddenly make their appearance, and, the difficulties in the circulation having disappeared, how it is that the worst attacks admit of rapid improvement. In consequence of an accommodation of the circulation to the altered calibre of the vessels being possible, though a complete restoration of integrity in the walls is hardly to be expected, we can understand how it is that, after an apparent restoration of function, there is still the constant liability to more or less disturbance.

The discovery of these changes in the cerebral vessels has, therefore, not only filled up a gap in the pathological anatomy of cerebral syphilis, but has also really for the first time explained the whole history of the disease.

To establish the syphilitic character of a disease of the

brain, it is plainly of the greatest importance to discover a previous infection, and the existence of other signs of past or present syphilis. If a patient admits that he has had syphilis, or is suffering from indolent buboes, syphilitic eruptions, or the remains of such, from affections of the mouth or throat, or their cicatrices, from bony tumours, periostitis, affections of the iris, ozœna, or hard cicatrices on the genitals, we should be quite justified in attributing any suspicious weakening of the cerebral functions to a syphilitic disease of the brain. Neither ought we to disregard other less trustworthy signs, such as slight enlargement of the glands in the neck, the nape, the elbows, or elsewhere, a marked defluvium capillorum, or a dry, lustreless character in the hair, very foul breath (without disease of the teeth, stomach, or lungs, and with fair cleanliness of the former), the existence of a dry glossitis, etc. A remittent febrile condition, such as sometimes accompanies the acute exacerbations of syphilis, may now and then assist the diagnosis of brain symptoms, especially if the stupor outlast the fever.

The accounts which patients give of their previous illnesses are certainly very often delusive. And, moreover, it is not unusual for cerebral syphilis to make its appearance such a very long time after all other symptoms of syphilis have been seemingly completely cured, or it is, perhaps, accompanied and followed by such ambiguous and slight indications of the specific disorder, that the diagnosis may not be aided at all, or only deceptively so, by inquiries in this direction.

On the other hand, a person subject to syphilis may be attacked by cerebral troubles which are quite unconnected with his constitutional malady, therefore, when such a person exhibits cerebral symptoms, such as an apoplectic seizure, an epileptic or maniacal paroxysm, or their consequences, it still remains an open, and, therapeutically, a most important question, whether the cerebral symptoms depend upon the syphilis, and, if they do, to what extent. It not uncommonly happens that, besides syphilis, there are one or more other factors present, which are in themselves sufficient to give rise to the severest cerebral symptoms, such as hereditary predisposition, drinking habits, a wild, dissolute mode of life, excessive mental strain or psychical influences, traumatic or

toxic agents, cold and solar heat, diseases of other organs, which are easily followed by mischief in the brain (heart, arteries, rectum, liver, genitals, etc.); and it is then that it may be especially difficult to estimate the various influences, and distinguish the one from the other, and give to each its proper share of importance. It may very well happen that the disease in the brain originated from some other source than syphilis, and that its shape and course have been influenced by the specific disorder. Consequently, we ought not to consider the existence of morbid cerebral symptoms anterior to the syphilitic infection as evidence against the syphilitic nature of subsequent cerebral symptoms. Thus, a private patient of mine had had repeated attacks of fainting, and other nervous disorders, ever since childhood. With him the cerebral syphilis began with faintings, which got gradually deeper and deeper, and of longer duration, and were joined by other symptoms of the specific cerebral mischief.

And, again, changes in the brain due to syphilis may have occurred, but remain latent, until the brain gets some well-marked injury affecting its activity, when the syphilitic cerebral disorder will stand forth in all its characteristic forms. It began in this way with a patient of mine, who had had syphilis fifteen years previously. The symptoms commenced three weeks after a fall on the head, which left him insensible for a whole day. There were topical cramps in the left arm first, and afterwards the whole group of symptoms which is peculiar to this disease came on. He was cured by mercurial inunction.

We must, therefore, endeavour to establish the diagnosis as firmly as possible from the character of the nervous symptoms. We ought at least to arrive at such a probability as to the syphilitic nature of the disorder as will justify the employment of a specific mode of treatment.

There is no such thing as a single nervous symptom which is pathognomonic of or peculiar to the syphilitic nature of this affection. On the contrary, any of the usual anomalies of function which follow a disturbed brain or spinal cord may also occur in cerebral syphilis, and any one symptom seen

in the latter disease may also occur in the non-syphilitic disorder.

Syphilis brings about disorders of sensibility of every kind and degree, from the slightest abnormal sensation to the most raging pains, from a scarcely noticeable weakening of sensation to complete anæsthesia in every organ; also disturbances of mobility of every form and intensity; every kind of involuntary movement, even epileptiform spasms, and the most extreme rigidity of the muscles; loss of power in every form and position, from the slightest weakness and unwillingness in the muscles to complete paralysis, and this in any cerebral or spinal motor nerve; also sleeplessness, as well as lethargy, in all their modifications, to the deepest coma; also psychical disturbances, from an abnormal excitement to a senseless delirium; and all degrees and kinds of mental debility, as regards memory, disposition, understanding, will, or the deepest feelings, to complete imbecility and abulia.

There is nothing characteristic of syphilis in the form of these cerebral and spinal symptoms, any more than there is in their intensity.

Still, sometimes a single symptom will excite our suspicions, though there may be no certainty.

It is, first of all, suspicious when a person exhibits especially severe nervous symptoms, for which we can assign no cause. But we must be very careful how we draw our conclusions on these grounds: every circumstance of the case must be carefully inquired into. There is no doubt that severe nervous attacks very rarely or never occur suddenly and unexpectedly to strong persons, especially males, with a well-constituted brain and nervous system, unless there is some hereditary predisposition, evil habit, preceding disturbances of the health, or external or internal injurious influence. The vascular and nervous systems, so sensitive in those with hereditary predisposition, and so easily affected by toxic, infectious, or mechanical agents, exhibit a remarkable power of endurance, and a very slight disposition to change in the healthily constituted. Consequently, an apparently causeless onset of nervous symptoms, especially if it be of a severe character, is always open to suspicion. Of course, it may be due to non-syphilitic

growths; but, to say the least, these are infinitely rarer than the syphilitic affections, whilst their symptoms gradually develope themselves, and, by their definite grouping, point to a certain site.

Thus then syphilis may call forth all kinds of nervous symptoms. There are, however, a number of peculiarities which it gives rise to with special frequency, and the presence of which, with nothing else to explain them, may cause unusual suspicion.

It will not be superfluous to indicate the character of at least some of these peculiarities, although it is impossible to describe them exhaustively in a few words.

The external appearance of a person suffering from cerebral syphilis is very commonly most peculiar. The features are flabby and vacant, and the expression fatuous, although the intellect may be by no means strikingly weakened. The eye is not fixed, but is somewhat staring. The carriage is loose and stooping. When he walks, even though there may be no paresis, the head and upper portion of the body fall forwards, almost as though he were suffering from paralysis agitans. In some very severe cases, there is a kind of tendency to run, *i.e.* the body is bent and carried onwards, so that it is difficult for the attendant to hold the patient. In the greater number the gait is uncertain, staggering, and shambling, though there is no loss of muscular power.

Maniacal outbreaks of extreme severity, with senseless ravings, are common enough. But the usual thing is a fatuous condition, varying from the slightest vacancy to the most complete imbecility. It is seldom that we can find no traces of fatuity in a patient suffering from cerebral syphilis: usually also the memory is weakened, and recollection and the current of thought are slow and lingering. There may be a tendency to weeping and laughing—a condition like hysteria—capriciousness, childish behaviour, a certain pusillanimity in an individual who exhibits but few other important disturbances of the brain, and whose psychical condition when he was healthy gave no indication whatever that he was weak in this direction; or the childish, hysterical condition may persist during the remissions, and even whilst real improvement is going on in the severer symptoms. With this change of character we may have a perverse

obstinacy, which, however, yields to proper treatment, and so does a tendency to outbreaks of temper from slight causes.

Amongst the most frequent signs of cerebral syphilis are attacks of giddiness, faintings, and apoplectiform seizures. The last comes on with or without hemiplegia, and shows every grade of intensity. It is in these apoplectiform seizures that the diagnosis is often extremely difficult. Still, owing to the mode of treatment being involved in it, it ought to be made as positive as possible. No doubt the character of the attack varies much, but we must depend upon the accompanying symptoms, the mixed character of the paralysis, and the course of the affection for the correctness of our opinion.

In severe cases we usually get, at some time or other, more or less torpor, sometimes the deepest coma, which may come on at the commencement of the symptoms, or accompany an apoplectic, maniacal, or epileptic fit, or may make its appearance during the progress of the disease. This torpor sometimes lasts but a short time, but frequently it continues for several weeks, it may be even for months.

Blindness is pretty frequent. It often comes on quite suddenly, but, as a rule, it is transitory. Ophthalmoscopic examination—as far as my experience goes—mostly gives but negative results. Deafness is decidedly more uncommon, and moreover sometimes disappears with surprising rapidity. Still more rarely is the sense of smell affected. I can only call to mind a couple of cases in which the sense of taste was even temporarily lost.

We can often find a decided peripheral origin for the pain in the ordinary neuralgias of patients suffering from syphilis. But the severe headaches and backaches, and the neuralgias which come and go in several nerves at once, and whose distribution does not in any way correspond with the course of a nerve, must, on the contrary, be attributed to syphilis in the nervous centres or their coverings.

The exceeding frequency with which speech is affected in cerebral syphilis is especially worthy of notice. The anomalies indeed which occur are most varied.

True aphasia is certainly not uncommon, but it is not by any means so frequent as some other conditions approaching

aphasia. The fully developed aphasia, in which the patient can only make use of one or two words or short phrases, and employs them on all occasions, sometimes makes its appearance without the corresponding right hemiplegia, and not uncommonly disappears after a few days, may do so suddenly after only a few hours' existence, or be introduced or followed by other difficulties of speech.

An attack of cerebral syphilis, which ended in the incurable imbecility of a gentleman, began with a sudden loss of speech whilst he was on a visit. Although the speech returned immediately, and no other symptoms showed themselves, the patient became anxious, and set out for home. When he got to the railway terminus, he got up into a droschke, but at the same moment became aphasic, and could not give the street or the number of the house to which he wished to go. Fortunately he had a letter with the address on it in his pocket. He showed this to the driver, and when he got home his speech had completely returned. But it did not continue long; further symptoms came on, the aphasia returned, and permanent difficulty of speech established itself.

More frequent than pure aphasia is the loss of a limited number of expressions, syllables, and letters, which the patient is, as a rule, also unable to write. This partial aphasia is usually amnesic; but it is also very common for the patient to get into a kind of habit of omitting words, syllables, and letters in his speech or writing which he is quite able to repeat or copy. Sometimes, after several vain attempts to recover a word or syllable, he will be successful at last.

The paraphrastic mistake of words, syllables, and letters is also very common, and from a kind of habit it sometimes develops into quite a characteristic peculiarity.

A stammering, stumbling, and slovenly mode of speech is pretty frequent amongst patients suffering from syphilitic disease of the brain; but in almost all who speak we may notice at least a slight, and sometimes a strikingly marked slowness and delay in the speech, and this is such a common symptom in cerebral syphilis that one might be inclined to denote it by some special term—perhaps "bradyphasia" might be suggested. This symptom may or may not be accompanied by delay in the flow of thought.

Frequently difficulties of articulation accompany and modify the other difficulties of speech.

A weakened intelligence may also complicate the difficulties of speech. It may often be noticed that there is a change in the character of the difficulties of speech in the course of the affection. There was a patient lying in the hospital some years since suffering from cerebral syphilis, who was, at first, persistently silent, and only answered "Yes" and "No," although he usually employed these words correctly. After that he lost his voice completely for three weeks, and was then obviously very absent and apathetic, but not comatose. When he again took an interest in things around him, he was aphasic, and answered all questions with the words "eine Kanne." He became better, but was somewhat fatuous, and now spoke like a little child, omitted all connecting words (Bindeglieder), used the infinitive, arranged the sentence in a childish way, and made a mistake with some syllables and letters.

Spasms of every extent and intensity are a very frequent feature in cerebral syphilis. Sometimes there are only partial fits; sometimes short, widely-spread quiverings; sometimes tetaniform or epileptiform seizures. The last kind of attack is exceedingly frequent, and varies much in character; it is always a sign of severe mischief. These epileptiform convulsions are usually not only stronger than the convulsions of true epilepsy, but they also last longer; they may continue almost unbroken for hours or days, and recur at short intervals. Twenty and thirty epileptiform seizures in one day is not an uncommon occurrence. During these attacks, sensibility to external irritants and consciousness are by no means always annulled, and we may easily see that patients suffer all the time the fit is upon them; sometimes also they have a dull recollection of the attack.

The partial contractions, which may appear in any group of muscles, but which do so most frequently in the biceps and rectus externus, are very peculiar. The active and passive mobility is fully retained up to a certain point. The forearm, for instance, can be easily flexed and extended within a certain range; but beyond a certain point all further extension ceases, because the biceps will not relax itself any further. This symptom usually lasts a very long time, is but little accessible

to therapeutics, and may persist after the other symptoms have disappeared.

The motor symptoms of paralysis in cerebral and spinal syphilis are exceedingly various, and take many different shapes. Paralyses of the cranial nerves are particularly frequent. The oculomotor is most frequently affected, then the abducens, and then the facial and hypoglossal. Topical paralysis of limited groups of muscles, and even of single muscles at the extremities, is, moreover, frequent, and so are complete hemiplegia and paraplegia. The latter scmetimes runs upwards in a remarkable manner, but it is very often added to some previous and quite local paralysis. In one case, which was repeatedly under clinical observation, and died of bronchitis, after seven years of most varied symptoms of cerebral syphilis, with intermittent but well-marked improvement, the illness began with deafness, then there was severe cranial neuralgia, and, one year after, a painful contraction of the left arm. Four years later there were spasms in the right arm, with traces of paralysis. Seven weeks after this an ascending paraplegia began, which, step by step, spread over the legs, bladder, rectum, arms, and neck. On the tenth day the tongue was paralysed, on the eleventh the œsophagus, on the twelfth the diaphragm, and on the thirteenth there was ptosis of the right eyelid. The patient's respiratory movements were now quite feeble. He lay almost like a corpse, though he was quite conscious. The energetic employment of mercurial inunctions counteracted the advance of the paralysis. Eight days afterwards the improvement was quite striking, and four months after the beginning of the ascending paralysis the man was able to walk again, with the help of a stick. But we do often get, more especially in connection with spinal syphilis, permanent paraplegias and tabetic forms (with loss of equilibrium when the eyes are closed)—the latter no doubt usually impure, and soon mixed up with wasting and real diminution of muscular power.

The involuntary passing of the urine and fæces is a frequent occurrence, without there being any paralysis of the sphincters, stupor, or delirium. It often appears to be more like an expression of listlessness, and want of a sense of cleanliness, when a patient allows his urine and motions to

pass from him in bed. Occasionally, when unwatched, he will urinate in his boot, or on the floor, and pass his fæces on a piece of furniture. And this will not uncommonly be the case with a patient who presents no marked psychical alteration.

The generative functions are almost always altered. Patients, without any considerable psychical disturbance, and no paraplegia, usually exhibit a striking loss of sexual inclination, and often feel suddenly impotent, or, at all events, frigid. On the other hand, the sexual desire is usually enormously exaggerated during the maniacal paroxysms; sometimes even in the fatuous condition, when the patient is indifferent to everything else, even to the taking of nourishment, he will get excited at the sight of any female form, or masturbate day and night. There is now lying in the private ward a semi-fatuous young man suffering from cerebral syphilis, who, despite every precaution, brings about five or six or more ejaculations during the day. The manipulations are continued until sleep is pretty deep; and, even during complete coma, the patient is accustomed to hold his hands on his genital organs. Frigidity and satyriasis often alternate with one another. The former occurs whilst the patient is improving, and the disease becoming intermittent; the latter appears in the more severe forms, and during the exacerbations.

An extremely noteworthy peculiarity of syphilitic nervous symptoms is, that they frequently appear, so to speak, in a fragmentary manner. Heubner has very well indicated this in his paper, where he says that the characteristic peculiarity of the general (syphilitic) cerebral troubles is, "the half-and-half, incomplete nature of the severe symptoms, the impairment of the consciousness without its entire destruction, the loss of voluntary activity without the complete abolition of all volitional impulse, the half-awake, half-asleep, and half-dreamy condition of the patient." So we find an unexpected flow of thought going on during an apparently deep sleep, epileptic spasms accompanying the retention of consciousness; torpidity of movement, in conjunction with indications of considerable strength; and the rigidity of a single muscle beyond a certain point only, up to which it can be moved quite freely, either actively or passively.

On the other hand, the intensity of certain symptoms is sometimes enormous; for instance, the wildness of the mania, the depth of the dementia, the acuteness of the pain and convulsions, the strength and variety of the movements in syphilitic chorea, the inflexibility of a contracted muscle, and the completeness of a partial paralysis.

In the diagnosis of the syphilitic nature of the disease, the combination of the several symptoms is of much greater service than the character of a single symptom. In other diseases of the brain we get well-known combinations of different groups of symptoms; for instance, collective symptoms may be combined with diffuse, right hemiplegia with aphasia, headache with paralysis of the extremities, and the like. But these combinations are explained by the site of the mischief. In syphilis, on the contrary, we get combinations which could not have been brought about by a disorder having a limited site, and which we do not find in other diseases of the brain and spinal cord.

As examples of combinations which occur pretty frequently, I may mention, one-sided ptosis, with mania or stupor; ptosis of one side and paralysis of the abducens of the other; oculomotor and facial paralysis of opposite sides; general weakness and torpor with insignificant hemiplegic symptoms; aphasia with left or changing hemiplegia; convulsions with paralysis; and various severe cerebral attacks with choreic movements.

The combinations are sometimes limited, but sometimes they are uncommonly numerous. In the latter case, we have to deal with not only a more severe, but a more dangerous affection.

The subsequent history of the disease offers still more decided evidence of its syphilitic nature than the single symptoms, or their groupings, do.

The symptoms of cerebral syphilis are sometimes permanent (those of spinal syphilis are so almost always), and only alter in the character of their gradually increasing intensity or combinations; or, if they are improving, in the character of their improvement; but they are exceedingly

often more or less transitory and variable, and change about one with the other in a manner quite peculiar to this disease, hysteria excepted. Many patients will exhibit in succession almost all the chief symptoms of cerebral syphilis; in others, the troubles are limited to a few groups of symptoms.

Accordingly, we cannot look for any one peculiar course in this disease.

Cerebral syphilis exhibits very many different types of history, but amongst them certain very peculiar rules are to be observed. This disease may hurriedly cause death, or lead the patient slowly to the grave; it may rapidly or gradually disappear; it may go away hesitatingly, or with relapses; and yet, despite all these differences, it exhibits such numerous peculiarities that the syphilitic nature of the affection can often be established with certainty.

I will endeavour to lay before you some of the most important of them.

In most cases we get a series of more or less persistent, insignificant, and frequently overlooked symptoms, which are often unnoticed, even by the patient himself; also some quite abrupt and very transitory attacks from time to time, *e.g.* fainting, sudden but quickly vanishing aphasia, lightning-like pain in a nerve. Owing to their momentary nature, patients usually pay but little attention to these attacks. We may call this the prodromal stage, although a false idea may be conveyed by this expression.

It is very exceptional for the severer troubles to come on in a gradual manner. As a rule, they make their appearance suddenly and abruptly—faintings, sudden aphasias, epileptiform or apoplectiform fits, sudden partial paralysis, blindness, for instance; or, at least, they rapidly increase in intensity, as acute melancholia and mania, weakness of will, stupor, severe headaches and neuralgias, choreic and cataleptic symptoms, and rapidly ascending paraplegia. In the latter cases the symptoms often convey the idea that we have to do with meningitis, encephalitis, or myelitis. It is a very common thing for a patient, who previously felt quite well, or only a little out of sorts, suddenly, or in a few hours, or, at the outside, in a day or two, to become very ill, and even to be in a most dangerous condition.

In this period of the sudden commencement of the severer symptoms we cannot usually say positively that the disease is syphilitic. From the symptoms we might quite justifiably diagnose hæmorrhage, embolism, meningitis, encephalitis, myelitis, rheumatic or gouty troubles, nervous delirium, or an ordinary epileptic paroxysm. It is only now and then that the cumulative character of the symptoms and the groups of symptoms, or the incomplete fragmentary nature of the attack, will arouse our suspicions during the first onset of the disease.

After the first sudden onset of the severer symptoms the disease will sometimes, though rather rarely, run on without a break from bad to worse, and lead to the death of the patient in a few hours or days. In such cases as these the syphilitic nature of the affection may not be established before the patient dies, and only the autopsy will reveal changes in the brain of such a nature and combination as by no means correspond with a sudden and abrupt attack in a previously healthy person, nor with a primary meningitis; much more frequently we find changes in the bones or membranes, or more especially in the vessels, which carry the disease back to a much earlier date, and by their wide-spread character exclude the idea of a purely local mischief.

But in the greater number of cases circumstances arise soon after the first onset which are calculated to draw the attention to the specific nature of the disease.

We may not infrequently observe during the course of a rapidly fatal case of cerebral syphilis, that symptoms, such as stupor, one-sided paralysis, aphasia, which in a severe localised disease (Heerderkrankung) of the brain, *e.g.* an extravasation or embolism, or an intensive meningitis, commonly persist until death, in this disease disappear spontaneously in a most striking manner, although the patient on the whole has not improved; further, that whilst the parts first attacked with paralysis are recovering their mobility, the paralysed arm again yielding to the will, the dropping eyelid rising once more, on the other side of the body we are getting a paralysis of the arm, a ptosis, an obliquity of the bulbus, and the like; thus there is an alternation of the severest symptoms such as no destruction, crushing, or persistent extravasation at the corresponding position could

or would have produced. Although death usually follows in such cases when they are left to themselves, it may sometimes be averted by an energetic specific treatment.

But this alternation of the symptoms is not always present; the acute symptoms may become aggravated to an alarming extent, epileptic attacks may follow one after the other, with stupor in the intervals; rapidly increasing imbecility may succeed maniacal excitement and sometimes alternate with it; coma may become deeper every moment, with only a moderate amount of fever; accompanied by a failing pulse, a rapidly ascending paraplegia, after involving the legs, rectum, bladder, arms, nerves of the chest and diaphragm, may finally include the cranial nerves and become general, or a paralysis primarily limited to one cranial nerve may attack the other cranial nerves and stupor follow. In such cases as this death may be expected at any moment. But an energetic well carried out specific mode of treatment, which is often selected simply from accessory circumstances or by good fortune, will, as it were in the face of death, arrest the catastrophe in the most surprising manner; the patient, who previously appeared to be *in extremis*, now, quite unexpectedly, shows traces of improvement; and if the same treatment be continued, after some eight to fourteen days the improvement will become unquestionable, and after that go on faster or slower but quite decidedly. In such cases as this we must not use our therapeutic agents timidly, and need not wait for complete evidence of the syphilitic nature of the affection. We can never do any harm with the inunctions or iodide of potassium, and if it turns out that the patient is syphilitic we shall then save him. Sometimes we get evidence of the nature of the disease in a supplementary manner or during convalescence, or from subsequent attacks.

But we do not of necessity get such extremely severe symptoms. Oftentimes after the attack we get a kind of *status quo* lasting for a longer or shorter time, which sometimes goes on uniformly, but sometimes with fluctuations or even intermissions; this is especially the case with neuralgias, choreic attacks, contractions, partial paralyses, and fragmentary stupor: or after the remission of the first symptoms others make their appearance. A longer or shorter time afterwards there

may be an aggravation of the symptoms either by fits and starts or gradually, or, especially after the employment of appropriate remedies, though now and then it may be spontaneously, an improvement occurs, which under the specific mode of treatment usually makes important and pretty rapid progress.

It is not uncommon for the paroxysms to pass away rapidly even without any treatment, no matter whether the symptoms be severe or moderate. The deep swoon may vanish and give one the impression that it was a matter of no special importance, and the aphasia may disappear of its own accord in the course of a few hours or days. The mania may be speedily succeeded by a calm, and the epileptic attack pass over as epilepsy usually does. The hemiplegia, the local paralyses, the limited convulsion and neuralgias, may disappear wonderfully quickly. And it is in such cases as these that the nature of the affection, and especially its very great seriousness, are, as a rule, overlooked. But this recovery of the health is delusive, the disease has by no means disappeared because the symptoms have spontaneously vanished. If specific treatment has not been adopted we may safely look for fresh paroxysms within a short time. If we observe these cases very closely, we may often discover certain anomalies in their course which indicate the existence of a latent disease. And although everything may appear to be going on as well as possible, another sudden attack will make its appearance sooner or later, sometimes like the previous one, sometimes of a different kind, but with every fresh attack we get an accession of the severest symptoms. We ought not to feel the slightest doubt as to the syphilitic nature of the recurring attacks when the successive paroxysms differ in their character and site. The future of the patient will depend upon the recognition of their nature and the proper employment of the correct remedies.

Contrasting with the foregoing irregularly progressive cases there are others, and certainly not a few, which from the beginning pursue a slow and uniform course. But when these cases are carefully inquired into, we usually find that they began in some more striking disturbance, or at all events developed quietly and slowly after a—little noticed it may be—

paroxysmal symptom; in any case we must be prepared at all times for the sudden onset of complications and aggravations of the disease.

With the exception of the most rapid and inveterate cases, cerebral syphilis offers a good prospect of successful treatment, but with spinal syphilis our chances are much less. In many cases, as I have already mentioned, the improvement occurs spontaneously. But under such circumstances it is the exception for it to be permanent. Whereas, if mercury and iodide of potassium be energetically employed, we may calculate with fair probability that the severity of the symptoms will be lessened, whilst the greater number of the most acute attacks will be improved and not uncommonly removed; this mode of treatment will be much helped and really better borne by means of warm baths, for which I prefer an imitation of the waters of Aix-la-Chapelle, when I cannot send the patient to Aix-la-Chapelle itself. This much may be asserted, that if a person be suffering from severe cerebral symptoms his prospects of improvement and recovery are infinitely greater if he is suffering from syphilis than if he is not. I have seen a considerable number of cases of cerebral syphilis in both hospital and private practice which have run a favourable course; I have seen numerous cases come up from the country after vain attempts at their treatment had been made, and very quickly improve under the specific remedies, in conjunction with the waters of Aix-la-Chappelle and warm baths (Acrotothermen). In spinal syphilis these results are attained infinitely less often and completely.

Although cerebral syphilis, even in its most desperate forms, offers a fairly good prospect of restoration to at least a tolerable condition, we must be careful to avoid any erroneous notions with regard to a complete cure. Speaking generally, in many cases we can do nothing at all; in others, only a very incomplete abatement and temporary limitation of the trouble is possible. Then, again, syphilitic cerebral attacks in persons who exhibit an hereditary or other predisposition to mental derangement always make us fear lest the latter should be enhanced by the syphilitic affection, and break out and become permanent. Under all circumstances, even in the most favour-

able cases, there is always the danger of a relapse, which often occurs even after a very long period. In addition to all this, the brilliant cures, when carefully examined, are always found to be exceptions, and a complete *restitutio in integrum* is scarcely ever to be expected. Roughly considered, no doubt these individuals often seem to be rid of their troubles, and they believe themselves to be so too. They may have no remarkable grievances; they may be able to undertake a moderate amount of work once more; they may be sent out of the hospital as "cured." In private practice patients return to their occupation in great spirits.

But if we search carefully we find that the former freshness and vigour of the mind, the keenness of the judgment, the liveliness of the conception, the equality of the voice, the accuracy and readiness of the memory, the perfect fluency of the speech, the free-will and steadiness of the muscles, and the sensibility of the sensory organs, have all suffered considerable damage. Syphilis impresses an indelible character upon the organism, and every portion of the body which was more specially affected retains marked traces of the change it has undergone. In the best of cases the brain becomes prematurely aged. It would be interesting to learn how long, on an average, persons continued healthy after their seemingly perfect recovery from cerebral syphilis, how long they lived after the same, and what their final end was.

It still remains to be mentioned that there is one disease which exhibits a perfectly similar variety in its symptoms, and the attacks of which are often very difficult to distinguish from those of cerebral and spinal syphilis. This is hysteria. The difficulty is increased by the fact that, on the one hand, hysterical persons may become syphilitic, and that, on the other, under the influence of syphilis, a condition may easily arise which we may certainly look upon as hysterical; so that we may not only have to decide whether, in a certain case, we have to do with syphilis or hysteria, but we may also possibly have to say how much syphilis and how much hysteria there is in the case—a question which will no doubt often remain ununanswered.

Another form of disease, which may likewise present very

great similarity to syphilis, and moreover be very frequently combined with it, is alcoholism (Potatorium). Not only do the attacks of cerebral syphilis sometimes present the greatest similarity to deep intoxication, both as regards the general impression and the separate symptoms, but, in addition, many permanent symptoms are common to both cerebral syphilis and chronic alcoholism.

# SYNANCHE CONTAGIOSA [DIPHTHERIA*].

BY

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GENTLEMEN,—Before entering upon a discussion of the disease which we have to consider to-day, I must make a few preliminary remarks with regard to its nomenclature, and lay before you the reasons which induce me to seek out for it a name by no means new, but long ago forgotten—I refer to the term Synanche (or Cynanche). At present, strictly speaking, there is no uniform designation for this disease—none which is used in the same sense by all physicians without exception, and none which embraces all the various symptoms. The name introduced by Bretonneau is the one we most commonly use—Diphtheria or Diphtheritis. On the one hand, however, this term generally includes the idea that the course of the disease must always be characterised by the formation of real or apparent membranes (*διφθέρα*, a membrane), and this, as you will hear, is not of necessity always and everywhere the case. On the other hand, there is besides a difference of opinion on this point, the term Diphtheria or Diphtheritis being applied by some only to certain apparent coatings of pseudo-membrane adherent to, and not separable from, the mucous membrane, while other authorities, on the contrary, lay less stress upon the condition of membranes and pseudo-membranes than upon the place where these are to be found, and apply the above term only to that disease which leads to their formation in the pharynx. The last point is, that the same word is employed to express a certain condition of wounds and ulcers, irrespective

* The contents of two lectures delivered in February, 1874.

of the part of the body in which they occur, without regard to any possible relationship which may exist between this condition and the disease of which we are speaking. In short, the term Diphtheria is used sometimes in a clinical, sometimes in an anatomical, and sometimes in an etiological sense; and we find also that the same idea is not always associated with it even by those who use it in one and the same—that is, in an anatomical sense. You will easily see that, with such confusion in the use of terms, it is extremely difficult, indeed almost impossible, to come to an understanding with regard to the many points in dispute connected with the theories of the disease in question. The fact is that there is scarcely any other field of pathology in which the confusion is so great—a confusion which will last until a general determination is arrived at to abandon all terms at present conventional, though used in different senses, and to select another less ambiguous, and which will involve as little prejudgment as possible, and not compel us antecedently to adopt any particular idea as to points in dispute, or as to anatomical or etiological conditions. As a suitable term for the purpose, I propose the word Synanche, or Cynanche, which was used by the earliest medical authors to designate certain inflammatory changes, pains, and constriction in the throat.* Inflammatory changes, troubles in the organs of the neck, in the fauces or pharynx, or in both combined, are the material symptoms of this disease; and as there is no doubt of its contagious character, the designation Synanche Contagiosa appears to me to be thoroughly suitable. We signify therefore by this term, an acute and contagious disease, especially prone to occur in children, generally commencing with fever, and producing certain changes of the mucous membrane at the place where the respiratory and alimentary passages cross each other, or at those portions of the two passages nearest to this crossing, changes therefore in the mucous membrane covering the tonsils, the uvula, and soft palate, the root of the tongue, the larynx, and the air-passages, the posterior wall of the pharynx, or the nasal cavities.

* As to the meaning of the words "Kynanche" and "Cynanche," as used by the earlier writers, compare Virchow, "Die Krankhaften Geschwülste ii." p. 609. In the course of this lecture I shall, however, adopt the term "Synanche," as involving less prejudgment than "Cynanche."

As in every other disease, so also in the one before us, a certain interval is found to elapse between the time when the morbific cause first acts and the commencement of the visible symptoms—a period which, particularly in the infectious disorders, is termed the stage of latency or incubation. This, in synanche contagiosa, usually lasts from two to seven days, but it may, apparently in exceptional cases, extend to three or even four weeks.*

If we omit the consideration of very slight and doubtful forms, and those of an abortive character, we find that the outbreak of the disease almost always takes place with somewhat violent fever, and a rapid increase of temperature, possibly up to 40° C. It is true that we but seldom have an opportunity of observing this first rapid elevation of temperature, and particularly of following it with a thermometer; but its occurrence is indicated by the chills which are seldom absent in older children and adults. In younger children a decided attack of shivering is, generally speaking, but rarely observed—a fact to which I have drawn your attention on various occasions; at the outside we may notice a temporary coldness of the limbs, and a bluish colour of the skin. On the other hand, there are other symptoms of a reflex character, especially vomiting, and also convulsions, both of which, however, are less frequent in synanche than in the other febrile infectious diseases; the children also become fretful and sleepy, but are very apt to start up out of their sleep. All these symptoms are frequently overlooked or not attended to, and on that account the advent of the disease appears to be a little more definite and more insidious, whereas in reality this is not the case. Patients able to give information as to their condition complain in the very beginning, not only of all kinds of undefined troubles quite irrespective of the fever, but also principally of a pain in the neck and back of the head, and extending from these parts, especially on movement.

The fever precedes by a certain, though short, interval the local symptoms, so far at least as these are visible to the naked eye. Whenever the occurrence of an epidemic of synanche has caused an attentive examination of the condition of the throat at

* Conf. W. Claes: Die Diphtherie in Mühlhausen in Thüringen, 1865-1868. Inaug. Diss. Berlin, 1870.

a very early period, there is, generally speaking, nothing abnormal to be discovered, or at most indeed, a little redness and a slight catarrh, which is often in no sort of proportion to the initial fever. Usually only on the second, rarely during the course of the first day, the mucous membrane of the above-named parts, most frequently of the tonsils or uvula, of the soft palate or arches, is found to be the seat of certain changes to which are forthwith superadded other local disorders, *e.g.* pains in the throat, difficulty in swallowing, and such like symptoms. At the same time, the submaxillary lymphatic glands at the anterior or posterior border of the sterno-mastoid muscle are already found to be more or less swollen.

The local anatomical changes are by no means always uniform in their character; on the contrary, they exhibit differences of various kinds, as we also see in other infectious diseases. These differences may depend in part upon the quantity and condition, possibly also upon the stage of development of the contagium, in part also upon the general predisposition of the individual attacked, or of the particular organ in which the disease becomes localised—that is, produces anatomical changes which we can recognise. These latter I shall describe to you somewhat minutely, as the confusion which prevails in the study of synanche is in great part occasioned by the variety of the views held concerning these very changes.

Let us turn our attention, in the first place, to the structures of the pharynx. Here we find simple catarrh of the mucous membrane as the first and slightest form, of which it is not necessary for me to give any exhaustive description. This, in many cases, occurs quite by itself, and independently of any other symptom, and terminates in a few days in complete recovery, just like an ordinary catarrh, the result of taking cold, or like a so-called angina simplex. In fact, this, the simplest form, the catarrhal synanche, is certainly very frequently not regarded as such, but as an ordinary catarrh due to cold. But if, during an epidemic, particular attention be directed to the subject, we shall not remain long in doubt that we have before us a catarrh of a specific character. In your future practice you will very often meet with instances in which, either in a house or in a family, one or more children are suffering from the severe forms of synanche, whilst other members, especially

the adults, of their immediate family, living under precisely similar conditions, and having been certainly exposed to the contagion (no other cause of disease being demonstrable), are affected only by catarrh. You will also always hear it said, and find it reported in the statistical accounts of disease, that whenever the severe forms, the so-called diphtheria or croup, are prevalent, and cause great mortality among children, the cases of simple angina are greatly increased in number. All this points to a connection subsisting between these various forms. We may also add that swellings of the cervical glands generally occur in the purely catarrhal synanche, though not to the same extent as in the other forms; and, lastly, that it very frequently happens that some one of the following forms developes itself subsequently to, and side by side with, the catarrh. We see, in the second case, that the mucous membrane is in a state of catarrh, and exhibits here and there small grayish-white membranous patches of a round or oblong form, which can be easily rubbed or stripped off, and under which the mucous membrane appears quite entire. The tonsils, the arches of the palate, and the uvula are the most frequent seat of these deposits, which are usually visible for a short time only, perhaps a day, seldom more, inasmuch as either the affection subsides, and at most only the catarrh remains, or a third form, of which I shall presently speak, becomes developed. If you remove a fragment of membrane from the pharynx of a patient, and, while still recent, wash it well with water, and then make fine preparations by teasing it out or by making sections, and then place these under the microscope, you will find, especially after a careful addition of a dilute solution of potass, that it consists of nothing else but the more or less altered pavement-epithelium of the mucous membrane. Many of these epithelial cells, especially those in the deeper layers next to the mucous membrane, are still in a good state of preservation, others are ragged, and in various stages of disintegration, so that their outline is still just visible around the large nucleus. The cells are often in close connection with each other, thus forming a kind of network, which is filled with opaque granular matter, until at last nothing remains but a finely granular mass, in which here and there an epithelial cell or a nucleus may yet be found. The only characteristic, perhaps, which distinguishes

these shreds of epithelium from the normal structure is, that in the former the substance of the cells, protoplasm and nucleus, appear more decidedly and darkly granular, abounding in and covered by very fine granules. Such granules are also often found outside the cells, or as the remains of the cells where these have perished, and they may be found singly or collected together in groups. They are regarded, in part at least, as the lowest stages of development of vegetable organisms, as fungi, and are connected with the disease. We shall have more to say on this subject by-and-by.

It is a fact of particular importance, and one which cannot be sufficiently insisted upon, that in these shreds there is certainly no indication of an inflammatory process. The epithelial cells show no trace of proliferation of the nuclei or division, nor do we find pus-corpuscles in or between them in any appreciable quantity; at most, an isolated cell of this kind, or a so-called mucous corpuscle may be found, which doubtless has, so to say, by accident come there from the secretion of the neighbouring mucous membrane in a state of catarrh: the pus-corpuscles certainly do not form an essential constituent of these shreds. I beg you to keep this steadily in mind, as it is of importance for the correct estimation of the character of these membranes, and for distinguishing them from structures which resemble them in outward appearance.

3. From the type of the disease just described a very gradual transition takes place towards a third form, in which, at various spots in the upper part of the throat, we find, as before, flat, grayish white patches. These, however, are not loosely adherent, and when they are removed the mucous membrane is not found to be entire; but even the unaided eye can discover lesions. The patches are firmly connected with the subjacent ulcerating tissue, and only assume the appearance of a coating or membrane because they form the cover of a shallow ulcer which extends itself superficially. The attempt to remove them always causes pain and bleeding, though this latter is slight. It is scarcely ever possible to detach them as a connected membrane; this can be done only when the covering is loosened and raised by the regenerative processes going on beneath it, before it breaks up, and is converted into *débris*. For this very reason it is not easy to give an explanation with regard to the

structure and origin of these pseudo-membranous patches in the living subject; we have mainly to depend upon an examination of the parts after death. On the other hand those loosely adherent epithelial shreds above described, since they correspond only to a slight amount of disease which *per se* does not cause death, are scarcely ever found in the dead subject, and probably for this reason are never particularly alluded to by pathological anatomists. We find by anatomical and microscopical examination, that wherever those flat ulcerations, exhibiting the whitish or ashy-gray adherent coating, are present, and the tissue of the mucous membrane in their immediate neighbourhood is densely infiltrated with round-cells (pus-corpuscles), and nuclei extending deeply into the submucous connective tissue and even beyond it, there are in addition regular extravasations of blood of greater or less extent, which are looked upon as the consequence of a compression of the vessels caused by the infiltration and of the blood-stasis resulting therefrom. The tissue superficial to the infiltrated part perishes (whether in consequence of this blood-stasis or from any other causes may remain for the present undecided), and then forms a soft slough, which is still more or less closely connected with the subjacent and surrounding tissue. This slough consists, therefore, of a confused, tangled mass, in which are to be found epithelial cells of various stages of age and development, and in all possible grades of decay, even so far as to be undistinguishable. In addition to these, the deeper layers contain pus-corpuscles, then blood-corpuscles and the free colouring-matter of the blood, in addition to molecular detritus and the above-mentioned fungus-organisms. You can give no better illustration of the origin and nature of this pseudo-membranous slough than by comparing it with the gangrenous slough of a bed-sore; but there is this difference, that the latter, on account of the histological dissimilarities of the integument, its thick layers of horny epithelium, its abundant supply of elastic fibres, etc., has a denser and harder consistence, while the former is more delicate and soft, corresponding to the thinner and more delicate layers of epithelium. Acute sloughing or diphtheritic inflammation, diphtheritis, will be the best designation to apply to the process which induces the formation of these pseudo-membranes.

I have already stated that there is a gradual transition from the second to this third form; we often observe that a spot, perhaps on the tonsils or the soft palate, from which on the first examination epithelial shreds had been removed without difficulty, exhibits shortly afterwards, perhaps on the following day, a firmly adherent, flattened slough. We may also find, on examining such a grayish-white, island-like patch, that one end can still be easily pulled off, leaving the subjacent mucous membrane apparently quite unchanged, whilst the remaining portion firmly adheres, as a true soft slough, to an ulcerated surface which is very prone to bleed. It certainly often happens that only this latter form, the adherent pseudo-membrane, is actually noticed, possibly because the time has gone by when the loose shreds were present, or because these latter never made their appearance, but on the other hand, ulceration and sloughing occurred from the very commencement, a circumstance which generally indicates a more intense form of disease, and a more rapid course of the whole process.

You will not wonder that the ulcerated portions of the mucous membrane of the throat with their pseudo-membranous sloughs are very apt to become the seat of putrefactive changes, for nowhere more than here is there such a combination of all the conditions for putrefaction, that is, the fœtid decomposition of nitrogenous materials. Warmth, air, moisture, decaying animal tissue, muco-purulent secretion, possibly also some adhering remains of food—can we imagine any combination more favourable for the production of putrefaction? We certainly require no delicate organ of smell to notice at the very commencement, when the patches of one or another kind appear, that disagreeable odour which is also found to a slighter extent in persons who neglect to cleanse the mouth or who have carious, dirty teeth. But under certain circumstances, such as, for example, when the putrefaction is promoted by unfavourable external conditions, moist impure air, underground dwellings and the like, or when more considerable layers of the mucous membrane become mortified in consequence of the deep extension of a great and rapid infiltration in which much blood is effused which saturates the decaying tissue—under these circumstances, the process is not confined to the formation of a superficial slough, but the entire parenchyma, and especially

that of the tonsils, breaks up into a dirty, grayish-brown or reddish-brown, fœtid, rotten pulp, which easily bleeds, and which also opposes no resistance to the least touch. Here then you have before you the pronounced type of moist or fœtid mortification or gangrene, that is, a necrosis with putresence of the necrotic tissues. You may, if you like, regard the gangrene as the fourth anatomical form in which synanche shows itself, though, strictly speaking, it does not differ substantially from the foregoing third form, of which it is only a higher grade.

Other forms of disease of the structures of the throat scarcely ever occur in synanche, and what you find described under different names by various writers as croupous or diphtheritic inflammation of the throat, or as angina gangrenosa, you will readily recognise as belonging to one of the above-mentioned forms, if you are not puzzled by the names, and if you reflect upon the real nature of the process, so far as it is evident from the descriptions.

The various affections of the larynx and of the upper portions of the air-passages, which, next to the throat, are most frequently affected, cannot of course be so well recognised by inspecting the patients. In children in general the laryngoscopic examination presents far greater difficulties than in adults, but in these latter, owing to the generally co-existing throat affection, it is almost impracticable, even with great dexterity, to obtain a view of the diseased larynx. We have to trust, on the one hand, rather to post-mortem examination to give us direct information as to the changes, and, on the other, to the disorders of function from which we infer the anatomical processes which are their cause. In these parts also simple catarrh frequently occurs during an epidemic of synanche, and as we must say on unprejudiced observation, in causative connection therewith; as the consequence, therefore, of the synanche infection. In the next place, the form designated as diphtheritis or acute sloughing of the mucous membrane is observed, and this by preference, if not exclusively, in the upper part of the larynx, upon the epiglottis, and down to the true vocal cords. A parenchymatous gangrene, of such frequent occurrence in the tonsils, never results from this superficial sloughing in the larynx, and the reason for this may well be that the advance of the disease is limited by the cartilage;

necrosis of this latter structure might at most be the result, though this scarcely ever happens, for the process is too rapidly terminated, and generally speaking by death. That form of epithelial disintegration which we see in the pharynx is unknown in the air-passages, not so much because, as I have already told you, it represents only a slighter disease and is less liable to come under the scalpel of the anatomist, but rather because of the more delicate condition of the epithelium, especially of the ciliated cylindrical variety below the vocal cords, an epithelium which, as is well known, is never detached in connected masses, and certainly not in catarrh, and which, therefore, is so very rarely found in the expectoration.

On the other hand there is a form, peculiar to the air-passages, and especially to the lower part of the larynx and the trachea, which very rarely, if ever, occurs in the pharynx. This consists in inflammation of the mucous membrane, with free fibrinous exudation, covering the surface as an investing membrane. Such a membrane, called a croup-membrane, consists essentially of coagulated albuminous material and pus-corpuscles. The former is deposited in concentric layers, which are connected with each other by many intervening filaments, so as to exhibit on transverse section a very delicate kind of network, the meshes of which contain one or more pus-corpuscles, or are often completely filled up by them. The coagulated albuminous material we call fibrine, a term which we apply to all albuminous substances, the casein of milk excepted, which coagulate spontaneously, *i.e.* without extraneous addition. Besides these two constituents, fibrine and pus-corpucles, with perhaps some isolated red blood-corpuscles, there are in the croup-membranes scarcely any organised elements. There are no epithelial cells, and even the fungus-organisms, which, as you can remember, are constant and abundant in the epithelial shreds from the throat and in the pseudo-membranous sloughs, are in the recent croup-membrane either absent altogether, or only found in decreasingly small quantities. Beneath the croupous deposit the mucous membrane seems to the naked eye to be quite unchanged, and in the dead body it is not always even swollen and engorged with blood to any extent. With the microscope the epithelial investment of the mucous membrane can often be recognised between the

latter and the croup-membrane; but sometimes not if, after some time, in consequence of saturation with exudation-fluid, it has become swollen and degenerated, and has consequently perished. The deeper tissue of the mucous membrane is filled with pus-corpuscles, but there are no extravasations of blood such as are usually formed in acute sloughing. If by degrees the fluid set free together with the pus-corpuscles upon the surface of the mucous membrane becomes less coagulable and remains fluid, so that a muco-purulent layer collects under the croup-membrane, the latter becomes dissolved, and, if the patient should still survive, may be coughed up as a connected membrane, or more or less softened and disintegrated.

Let me once more reduce to a few words the anatomical changes which synanche contagiosa causes. There is, in the first place, the catarrh of the throat and air-passages, which occurs independently, or as the forerunner and concomitant of changes in other parts. In the second place, a separation of the pavement-epithelium, in connected, membrane-like shreds, this being only observable in the pharynx. Thirdly, the acute sloughing, or diphtheritic inflammation, which occurs in the pharynx and upper portion of the larynx, and which may, especially in the tonsils, increase, until it becomes a gangrene invading the deeper parts. Fourthly and lastly, inflammation with free fibrinous exudation ("croupous" inflammation), which usually attacks the lowest part of the larynx and the trachea. One or another of these forms is also found, if the disease invades other portions of mucous membrane besides the above-mentioned tracts. Thus, catarrh of the nasal cavities and of the tympanum very frequently accompanies the various affections of the throat, and the catarrh may attack even the external auditory meatus, as I have several times observed during one of the recent epidemics. In severe cases, the acute sloughing or diphtheritic inflammation is found even in these parts, whereas, in my opinion, it is doubtful whether a free fibrinous exudation, a so-called croup-membrane, occurs here, although croup of the nasal mucous membrane has been often mentioned. On this point, however, further investigations are very desirable.

Diphtheritic inflammation of other mucous membranes has also been sometimes observed to occur during an epidemic of

synanche, and in connection therewith; for example, upon the mucous membrane of the conjunctiva (the so-called ocular diphtheria), or upon that of the genital organs. But we must not, however, conclude from this, that every diphtheritic inflammation of these mucous membranes, even when occurring endemically, is connected with synanche. The superficial sloughing, or diphtheritis, may develope itself upon the surface of any wound or ulcer, under certain local or general conditions, as I shall presently point out.

You will now be able to judge, that we neither agree with those who depend only upon the rough external condition of the membranous patches which appear on the mucous membrane, and talk of a croupous or diphtheritic inflammation according as these are loosely or firmly attached; nor yet with those who deny all difference between the membranes, or pseudo-membranes, in the throat, larynx, and trachea, and regard them all as one and the same exudation, whether called croupous or diphtheritic. You have heard that loosely adherent shreds are found in the pharynx which have nothing in common with an exudation; these are remnants of epithelium. The term exudation is just as little applicable to the membrane-like sloughs in the pharynx and upper part of the larynx, for that which is exuded in the process which leads to the formation of the slough, is situated in the tissue of the mucous membrane under the slough, extending at most to the lowest layers of which we find some of the exudation and of the nuclei and pus-corpuscles, especially when the slough has become detached, this being effected with a very superficial and slight suppuration. On the other hand, the croupous membrane of the trachea, as we have previously described it, is a positive, real exudation in the strictest sense, consisting principally of fibrine and pus-corpuscles, the two constituents which are alone characteristic of inflammatory exudation. The question as to the source and origin of these two constituents, to which lately numerous investigations have been devoted, may be here passed by. I will just briefly mention that E. Wagner, to whom we are indebted for very minute investigations with regard to croup and diphtheria, considers that the fibrinous network is the result of a peculiar (croupous) metamorphosis of the epithelium. It certainly seems difficult to explain in this

way the manifold arrangement in layers of the membrane, which may be ten times as thick as the epithelial investment, in the lower part of the larynx and trachea, where the thin epithelial covering consists altogether of not more than three or four layers of cells. We should be obliged to assume a succession of formations and metamorphoses of the epithelium, and a metamorphosis so rapid and complete that no trace of epithelium can be discovered in the membranes; or else an enlargement of the existing epithelial cells, up to ten times their usual volume.

I cannot sufficiently insist upon the differences in these membraniform structures, which are deposited upon and within the tissues; but I will by no means dispute the fact that these various processes not only occur side by side, but that they may pass directly, temporarily, and locally into each other. On the contrary, we find this to be the rule in synanche, and the fact of the processes occurring together demonstrates the existence of the etiological connection. I have already mentioned (p. 406 *et seq.*) how catarrh of the throat, the epithelial shreds, and the pseudo-membranous sloughs, appear in succession, or side by side; and the same holds good of the anatomical changes in the air-passages. In children who have died from synanche, with localisation in the larynx and trachea, the ordinary anatomical appearances are as follows:—Pseudo-membranous sloughs (diphtheritis) in the upper part of the larynx, the vocal cords being also involved; thence, downwards, a fibrinous superficial exudation ("croup"), extending more or less deeply into the trachea; and, still lower down, a catarrhal state, which is continued to the larger, or even to the smaller, bronchial tubes. But there is never any sharp boundary-line between these processes; and, in particular, it is in many, indeed in most, cases quite impossible to say where the fibrinous superficial exudation, the croup-membrane, ceases, and the pseudo-membranous slough, the diphtheritis, begins. Is that, however, anything very wonderful, and without a parallel in pathology? Do we not see exactly similar appearances in other parts of the body under the influence of violent inflammatory irritation? Many of you must have seen a burn of the skin produced by intense heat, perhaps by a red-hot piece of iron. Where the action has been most energetic you

find a slough; beyond that, an exudation in the form of a bladder, or vesicles, consisting of pus-corpuscles and an albuminous material, which coagulates on exposure to air on opening the vesicles, and which is, therefore, fibrine. Still further outwards you find desquamation of the epidermis of the reddened and swollen skin, in larger or smaller shreds. No one would apply the term exudation to these shreds of epidermis, which are the results of a more active cell-formation in the rete Malpighii, and a more rapid detachment of the older layers consequent thereupon. Finally, at some distance externally there is, at least macroscopically, no such desquamation visible, but only redness and swelling, which gradually lose themselves in the healthy parts around. Now, here you have, as a consequence of one and the same cause, acting only in a different degree, a great variety of appearances, side by side with, and directly passing into, each other; and there has never been any dispute or doubt as to the terms to be employed to designate them. In a certain stage, namely, before the detachment of the slough, it is here also difficult to say where the slough exactly ceases and the exudation begins. The differences between the processes on the skin and on the mucous membrane, the formation of vesicles, the absence of the catarrhal secretion on the surface of the spots, which are desquamating, or merely hyperæmic—all these depend upon the differences of structure of the two membranes, and are quite immaterial.

You see, therefore, Gentlemen, that the similarity of the morbid products is as complete as possible; but I will not assert that, as in this case upon the skin, so upon the mucous membrane, the sloughing corresponds to the most intense, the fibrinous exudation to the less intense, action of the morbific cause. For in the mucous membrane other conditions may determine the appearance of the diphtheritic inflammation in the throat as far down as the vocal cords, but of the croupous kind beyond these parts. We have to consider the differences in the structure and arrangement of the mucous membrane, in the thickness and quality of its epithelium, differences also in the function. Other very important points are, those which concern the external sources of mischief to which these parts are exposed, and especially those agencies upon which, as you

will by-and-by hear, it most probably depends whether an inflammation becomes diphtheritic or not.

We do not find in every case of synanche that the disease attacks all the above-named portions of mucous membrane, either simultaneously, or one after the other. On the other hand, the disease commences, in by far the majority of all the cases, on the tonsils, the soft palate, or even on the posterior wall of the pharynx, and, after that one or another form of inflammation has become developed, very frequently remains confined to these parts during the further course of the affection, and until it terminates. This occurs especially in older children and adults. Frequently, however, the inflammation extends to the posterior part of the dorsum of the tongue, then to the epiglottis, to the interior of the larynx, and further downwards into the trachea, and even into its branches. Cases are extremely rare, though their occurrence has been distinctly proved, in which the larynx is attacked without the previous occurrence of inflammation in the pharynx, with separation of epithelial shreds, or formation of a slough; or without the laryngeal affection being at least very rapidly followed by the affection in the pharynx ("croup ascendant"). The Baden physician, Ruppius,* was the first to show that, as a general rule, the more severe croupous-diphtheritic inflammations of the larynx are preceded by a severe affection of the pharynx; and a few years later, Bretonneau, of Tours, arrived independently at the same conclusion. It is now difficult to decide whether, in former epidemics of malignant laryngeal disease in children (so-called croup), the throat-affections were always absent; or whether they were overlooked, owing to insufficient examination. It by no means unfrequently happens that the inflammation extends from the structures of the throat, not in a downward, but in an upward, direction, into the nasal cavities, or simultaneously upwards and downwards into the air-passages. On the other hand, it is remarkably rare for it to spread from the pharynx to the œsophagus; but it somewhat more frequently happens that it spreads from the nasal cavities to the conjunctival mucous membrane. The converse

* Altenburger Med. Annalen 1822, Schmidt's Jahrb. xx. 1838. p. 211. und Friedreich, Krankheiten der Nasenhöhle etc. in Virchow's Handb der Pathol. v. 1. p. 442.

has also been observed, namely, that an affection resembling that of the pharynx, which leads to the formation of a slough in synanche, first appears on the conjunctiva, and then extends to the mucous membrane of the throat. How far, in this instance, a real synanche infection is at work, cannot at present be decided, but it appears to me certain that all the cases and epidemics of the so-called conjunctival diphtheritis were not connected with synanche.*

We now return to the clinical phenomena, the symptoms and their course, of which I shall give only short sketches, to supplement what I have already said. The symptoms present little that is peculiarly characteristic of synanche, and may be inferred simply from the local affection.

With regard to the fever, there is nothing typical. In this respect it differs from other acute infectious disorders. The increase of temperature is at first rapid, but is most frequently followed, in the course of a day, by a more or less marked decline, with free perspiration; and where the inflammation is of a slighter character, and does not extend over the pharynx, the temperature either remains normal, or rises a little in the evenings, until, after a few days, the disease comes to an end. In the more severe forms, the fever is subject to variations in its further course, and, as long as the disease is confined to the structures of the throat, corresponds nearly to the intensity and duration of the local mischief. It is therefore less in cases where there is only a separation of epithelial shreds than in the diphtheritic form; and here again it is less than when mortification or gangrene of the deeper parts ensues. In such cases, the fever may continue for a week or two, or even longer, while the detached sloughs are replaced by new ones, or the ulcerations extend superficially; and, if gangrene sets in, there is a septic fever in addition.

In cases where the air-passages are involved in the disease, whether from the very commencement, or in the subsequent course, the fever, apart from the first elevation of temperature, is extremely variable, and, in particular, it stands in no sort of

* Conf. v. Gräfe im Archiv f. Ophthalm. ii. p. 168. *et seq.* Jacobson daselbst vi. 2. 180. Hirschberg in den Verhandl. der Berliner Med. Gesellschaft 1867—1868 p. cxcvii.

proportion to the severity of the affection, or to the danger to life. You all know well how the invasion of the larynx is manifested by alterations of the voice, the peculiar, hollow, barking cough, the want of breath, which, in severe cases, very soon increases, generally in from three to five days, until there are all the characteristic symptoms of stenosis of the larynx. I will just draw attention to the fact, that the constant and gradually increasing difficulty of breathing is interrupted from time to time by real attacks of asphyxia, any one of which may be fatal. These attacks are caused by some mucus, or shreds of exudation or tissue getting upon the vocal cords, or becoming fixed in the slit, already small, which exists between them. The early setting-in, and the violence of the dyspnœa, are frequently aggravated by the swelling of the tonsils and by the coryza, when the nose is also affected. After such an attack of suffocation, or in consequence of violent efforts at coughing, or even with choking and vomiting, it often happens that portions of membrane are ejected from the air-passages, whereby at least temporary relief is obtained. Generally speaking, fresh formation of membrane takes place, the carbonic-acid poisoning incessantly continues, and asphyxia and death result after the disease has continued from five to ten days, rarely for a longer period. The fatal result is sometimes preceded by emphysema of the neck, resulting from rupture of the pulmonary vesicles, and escape of air under the pleura, and into the mediastinum and the cellular tissue of the neck.

In synanche with inflammation of the air-passages leading to fibrinous deposits and formation of sloughs, recovery takes place only in about ten or fifteen per cent. of all cases, irrespective of those in which operation is had recourse to, since, after separation of connected portions of exudation-membrane, or of softened and disintegrated shreds, a catarrhal stage of resolution is impossible.

Several circumstances may assist in accelerating the termination by asphyxia. Inflammation of the air-tubes, and of their larger branches, is often accompanied by a capillary bronchitis, with its consequences, atelectasis, and consolidation of separate portions of the lungs; or, in rarer cases, circumscribed pneumonic foci result, the inflammation being caused by particles of disintegrated tissue from the upper diseased portions

finding their way into the lung during inspiration. Moreover, embolic pneumonias may also originate in this way. Thrombosis takes place in the small veins in the neighbourhood of the diseased mucous membrane, and fragments of the thrombi get into the blood and reach the lungs.

Death seldom occurs from other causes than these various affections of the respiratory organs, which have just been noticed, at least while the disease is at its height, for, generally speaking, synanche, when confined to the throat, is one of the least dangerous of the infectious diseases. Such more rare causes of death are, septicæmia, which, as I have already said, sometimes supervenes upon gangrene of the tonsils; and uncontrollable hæmorrhage, when, perhaps, a large vessel has become eroded.

In very many cases of synanche, whether localised only in the throat or in the air-passages as well, albumen is found in the urine, generally as a temporary symptom, and seldom lasting longer than one or two weeks. Its occurrence is by no means always, as was formerly supposed, a bad sign as regards prognosis, for it may be due to various causes, not always serious, which certainly are not very evident in each individual case. In the first place, albuminuria may here have the same significance as in other febrile diseases, in which, when the temperature of the body is high, it occurs without serious affection of the kidneys, and disappears when the temperature becomes reduced. In these cases the urine generally contains only small quantities of albumen, and scarcely any formed elements. Another, and perhaps the most usual cause, is the occurrence of a parenchymatous nephritis, a swelling and fatty degeneration of the epithelial cells of the kidney, just as so frequently occurs in acute infectious disorders and blood-poisoning. But also in these cases, unless other changes are at the same time present, the quantity of albumen is inconsiderable, and by no means constant. In cases also in which the disease is localised in the air-passages, and dyspnœa results, the venous stasis which then takes place may cause the urine to become albuminous, and you may then have all the well-known symptoms of suppression of urine. Finally, in the fourth place, albumen, usually in a larger quantity, may be found together with blood and lymph-corpuscles and kidney-epithelium, as the expression of

an acute, diffuse nephritis, but in this case we have to deal, not with synanche, but with a scarlatinous process, which, like the former, becomes localised in the throat, and therefore is frequently confounded with it. Only the two last-mentioned kinds of albuminuria are accompanied by dropsy, which is scarcely ever observed in the first and most usual forms, or when it does appear is only very slight, showing itself, for example, in œdema of the ankles, in cases where there is great anæmia, lasting for a considerable time, during the stage of convalescence.

There is yet one very remarkable fact to which I must allude, examples of which are to be found during many epidemics of synanche, both in patients suffering from the ordinary symptoms of the disease, as well as in persons who had been quite free from such symptoms. We find, that is to say, that wounds and ulcers, situated neither upon the mucous membrane of the throat nor in the immediate neighbourhood, but upon the mucous membranes at a distance, or upon the integument of the body, put on an appearance resembling the acute sloughing process in the throat, and become diphtheritic. I will by-and-by explain to you the connection which, at least in my opinion, exists between *this* diphtheritis and synanche, and for the present I will only call your attention to the fact that a really diphtheritic inflammation, that is, one leading to the formation of a slough, is not present in every ulcer and in every wound to which the term "diphtheritic" has been commonly applied, the resemblance being in reality only external. In one instance, the pus being very thick and tenacious, and not removed from the ulcer with sufficient rapidity, may dry up so as to form a greenish, viscous coating, or from some cause or other the quality of the pus may be so changed that it coagulates spontaneously upon the ulcer in flakes and shreds, such as we do not see in ordinary good pus. Besides this, on ulcerated surfaces, especially if they have been subjected to actively stimulating treatment, we may find a superficial fibrinous exudation, a true croup-membrane, consisting, like the latter, of fibrine and pus-corpuscles, and which may also be removed in connected pieces of greater or less extent. All this, however, is not diphtheritis, a condition which actually exists only when a real portion of the surface of the wound perishes, and adheres as a pseudo-membrane to the deeper portions in the form of a gray, discoloured

slough. Your surgical studies will inform you that this true diphtheritis of wounds may occur quite independently of an epidemic of synanche, and particularly wherever unfavourable conditions are present, whether these be general and atmospheric, or individual and local; and you will also find that this disease has certain relationships with hospital gangrene, and may even pass into it.* And on the other hand, it is certainly not the case that every wound and every ulcer becomes diphtheritic during an epidemic of synanche, either in ordinary cases or even in patients suffering from that complaint.

Even the convalescent stage of synanche is accompanied by dangers to which many a patient who has so far recovered from the severest forms, may yet fall a victim. In the first place, there is often an obstinate and continuous loss of appetite, an insuperable repugnance to food, and this is frequently accompanied by nausea and vomiting. We meet also with cases in which death occurs suddenly, and it is only recently that any particular attention has been paid to these. After the special symptoms of the disease have run their course, at a time when the affections of the mucous membrane are undergoing resolution, or are even quite healed, and the general condition gives rise to no anxiety, a patient may suddenly collapse under symptoms such as those ascribed to a paralysis of the heart; the pulse becomes extremely frequent, only in very rare cases slower than usual, at the same time small and weak; the skin pale and cool, and without any other symptoms than extreme prostration resembling syncope, the patient rapidly dies, sometimes within a few hours, œdema of the lungs having in many cases supervened. As a cause of death, Hillier† and Mosler‡ have found fatty degeneration of the muscular substance of the heart; in other cases nothing more has been discovered than extensive coagula in the heart and large veins, which doubtless owe their origin to the weakened action of the heart. This enfeeblement of the heart's action, however, need not depend in every case upon rapid fatty degeneration, even though only microscopically demonstrable, of the muscular tissue, but it may also

* Conf. the lectures in this Collection by Hüter No. 22. and König No. 40.

† Diseases of Children. London, 1868. Virchow u. Hirsch Jahresb. 1868. ii. 645.

‡ Archiv d. Heilk. 1873. i. s. 61.

be due to an interference with, or total suspension of, the heart's innervation; to a true paralysis therefore, and, as the symptoms make us conclude, a paralysis most frequently of the inhibitory nerves running in the channel of the pneumogastrics, or situated in the heart itself, much more rarely of the excito-motor sympathetic fibres.

The assumption that at least in a portion of these cases of sudden death there is a real paralysis of the nerves of the heart, especially of the vagus, is particularly supported by the fact that paralyses of other kinds often occur during the convalescence from synanche, and indeed many weeks after the acute stage of the disease has come to an end, and when a remarkable degree of paleness and languor is the only remaining symptom. These paralyses apparently always commence in the throat, and extend to other parts with a certain regularity. In cases where they appear to take a different starting-point, the paralysis of the throat has been probably overlooked, and this is the more likely to be the case, inasmuch as the functional disorders caused by the paralysis, the difficulty of swallowing, the nasal character of the speech, have a certain resemblance to those which were due to the previous inflammatory and ulcerous affections of the mucous membrane, and might therefore be considered, on superficial observation, to be merely the remains of these. The soft palate, therefore, is the first part to become paralysed, and, as a consequence, the nasal cavities are no longer shut off from the mouth in the actions of speaking and swallowing. Simple inspection of the throat is sufficient to establish the diagnosis, for the arches of the palate and the uvula are found to be relaxed and dependent, and to be incompletely, or not at all raised when attempts are made at phonation, and to exhibit little or no reflex movement on being touched. If at the same time the closure of the aperture of the larynx is still normally effected (by the root of the tongue being pressed against the epiglottis, and this latter depressed), the manner alone in which articles of food; and especially liquids, regurgitate is characteristic of the complaint; for these pass quite noiselessly out through the nose without any signs of pain, and are not ejected from the mouth and nose with violent fits of coughing, as is the case when the food goes the wrong way, as it is called. Subsequently the nerves of the

larynx are also involved; and according as the superior or the inferior laryngeal nerves, or both together are paralysed, the sensibility and the reflex excitability of the laryngeal mucous membrane are destroyed, and the movements of the vocal cords interrupted or impaired in various ways. In such cases food and drinks easily pass into the air-passages, and may even give rise to serious mischief, pneumonia, for example. Next to these, the nerves of the muscles of the eyes, and especially the ciliary branches, are liable to become attacked, the consequences being defective accommodation (inability to perceive near objects distinctly). It more rarely happens that the paralysis extends still farther, so as to involve the facial nerve; the nerves of the extremities are somewhat more frequently attacked, but in no definite manner; and finally, it is only in very rare cases that the paralysis invades the nerves of the trunk, or those supplying the muscles of respiration, or, as already mentioned, those of the heart. Paralyses also of the nerves of special sense, especially amblyopia or amaurosis of temporary duration, have been occasionally observed. I need not describe to you the disorders which produce these various paralyses, I will only remark that there is generally a close resemblance between these latter and peripheral paralyses. In cases, therefore, where mixed nerve-trunks are affected, *e.g.* in the extremities, we find disorders of sensibility and smarting, rheumatic pains, but these latter more rarely than paræsthesia, numbness, formication, or complete anæsthesia for several or all degrees of sensation, in addition to the motor paralysis or even before this occurs, and these disorders of sensation usually disappear much sooner than those of motion. The application of electricity frequently yields the results which are characteristic of peripheral paralyses in a certain stage, namely, diminution of the faradic contractility of the muscles, with normal, or even increased galvanic excitability.

There is, however, another peculiarity of these paralyses, and one which distinguishes them from the majority of those of a peripheral kind; their advance is not continuous, but by fits and starts, as is shown by their manner of spreading above described, and then there is the additional fact that they do not usually attack one entire nerve-trunk, but only one or more branches thereof, and often only for a short time, and then

invade others. This can be often observed, especially in the eye, where sometimes one and sometimes another branch of the motor oculi nerve is attacked; and again, sometimes the abducens or the trochlear nerve. This peculiarity also explains why, in affections of the pneumogastric nerve, the trunk of which contains, as you know, nerves for so many different organs and functions, the paralyses may be of such very varied kinds, and so very dissimilar as regards prognosis. We also see why we cannot expect the same collection of symptoms which is observed to follow division of the trunk of the pneumogastric in the neck in animals, but only one or another kind thereof according as the paralysis involves motor, sensory, or reflex inhibitory fibres, fibres for the palate and pharynx, for the larynx, for the heart, or for the muscles of respiration, for the œsophagus, stomach, or duodenum.

All these paralyses, those only excepted which cause death by affecting the movements of the heart and muscles of respiration, generally tend to a favourable issue. The few severe and fatal instances, those especially in which, by degrees, almost the whole of the muscular system becomes involved, impress one more with the idea of a central than of a peripheral paralysis. These cases are sufficiently explained by the discoveries of Buhl and Oertel. Various changes, proliferation of nuclei and hæmorrhages, were found in the spinal ganglia by the former observer, while Oertel found similar changes in the large nervous centres and their membranes, and in the gray substance of the spinal cord. On the other hand, it is difficult to say what may be the precise cause of those really peripheral paralyses which form the great majority. Their mild nature and changeable character compel us to assume that we have to deal here with a disorder which is only slight, and which is rapid both in its advent and in its departure; and it follows, from what I have laid before you with regard to the supervention of the paralyses, that they must proceed from the palate, and the posterior wall of the pharynx; that they must be a peripheral affection of the nerves, advancing from these parts, and one which we may therefore designate as neuritis migrans.* The

* Conf. Möller, Deutsche Klinik, 1863. Nos. 42-46, and H. Weber, Virchow's Archiv, xxviii. p. 489. We are as much entitled to use provisionally the term Neuritis in the case before us as we are to apply the word Myelitis

insidious way in which certain morbid processes, especially inflammatory irritants, invade the connective tissue, affords a ready explanation of the typical extension of the paralyses before us.* The nerves of the soft palate and pharynx come, for the most part, from the pharyngeal plexus, in the formation of which the glossopharyngeal, the pneumogastric, and branches from the superior cervical ganglion of the sympathetic take part. Along these nerves, in their connective-tissue sheaths, and in the loose connective tissue which surrounds them, the morbid process gradually creeps on, along the terminal branch, upwards to the carotid plexus, and hence to the ciliary ganglion, to the abducens, motor oculi, and trochlear nerves, downwards to the cardiac plexus, as well as along the various branches of the vagus, to the spinal accessory nerves, etc. In this way we can easily understand the advance of the morbid process to the spinal ganglia and the spinal cord.

The question now arises, what connection have these paralyses with the synanche? A very special and peculiar relationship has been often assumed to exist between them, and those paralyses have been regarded as specific of synanche ("diphtheritic"), in the first place because of their frequency, and then because of the peculiar manner in which they spread, and lastly, because similar paralyses have been sometimes observed after the "diphtheritic" ulcers of the skin, to which I have previously alluded. It is true that these considerations certainly suggest the notion that these paralyses are of a specific character; but, on the other hand, recent years have afforded us innumerable experiences with regard to the setting-in of paralyses and neuroses, which diminish very considerably the force of those considerations. In the first place we now know—and Gubler has the merit of first having drawn attention to the fact†—that

to those various affections of the spinal cord, in which an inflammation in the ordinary and typical sense is likewise not proved.

* Conf. the Lectures by Leyden No. 2 (p. 16 *et seq.*), Billroth, No. 4, and König, No. 57.

† Archives Générales 1860, i. Surmay ibid. 1865. i. Nothnagel Deutsches Archiv f. klin. Med. ix. 480. S. Huss, Statistique, etc., du Typhus et de la Fièvre typhoïde. Paris 1855. p. 225. Bavailler, Du Typhus épid. à Toulon. Paris 1861. p. 255. Murchison, A Treatise on Continued Fevers. London, 1873, p. 205. The last-mentioned (p. 201) communicates a case of paralysis occurring during recovery from typhus fever; the patient

even in convalescence from other acute diseases, such as typhoid, typhus, and small-pox, paralyses are by no means of rare occurrence, and that they may be as transient and variable as those occurring after synanche. It is certainly extremely doubtful whether the latter are really more frequent in proportion—that is to say, whether a larger percentage of convalescents from synanche are attacked by paralysis than of those, for example, recovering from typhus; we have at least no numerical proof of this, and such a proof is extremely difficult. It may easily be that the paralyses following synanche come under observation more frequently only because this disease is now very widely spread; and in many large cities, such as Berlin, for example, strictly speaking, it is never extinct. As regards the peculiarity of its spread, from the explanation I have previously given you, you are now able to conclude that this is really due to the circumstance that synanche usually becomes localised in the throat. If another portion of mucous membrane were first attacked, it, and not the throat, would be the starting-point for the paralysis. In fact there is a series of paralyses, which occur in consequence of more or less extensive ulceration of various mucous membranes, and to which various names have been assigned. Thus there are dysenteric paralyses, preceded by ulcerative processes in the large intestines; puerperal paralyses, when the mucous membrane of the genital organs has been the seat of disease in lying-in women; urinary paralyses, when the mucous membrane of the urinary organs has been diseased; many other kinds, also, not as yet precisely indicated, belong to this category of paralyses. All these paralyses have their starting-point in the portion of mucous membrane primarily attacked, whence they spread continuously, or by fits and starts;* and they may involve the same nerves which are characteristically affected in synanche. There is a great similarity, as regards their spread, between these paralyses and those which sometimes supervene upon ulcerations of the inte-

had been treated by blisters to the neck, and the blistered surfaces had sloughed. The paralysis exhibited all the characteristic features of the form termed "diphtheritic;" there was impairment of vision, difficulty of swallowing, remarkable slowness of pulse, dyspnœa, &c.

* Conf. Leyden Ueber Reflexlähmungen. Vortrag No. 2.—Feinberg Berl. klin. Wochenschrift 1871. No. 41 *et seq.*

gument, and which have been termed "diphtheritic," from the fact that the affections of the skin had a really diphtheritic character—that is to say, presented all the signs of acute sloughing, or only such as resemble this, a greasy-looking layer of pus, or a fibrinous exudation-membrane (conf. above, p. 421). Again, these paralyses do not commence in the throat and larynx like those which are usually seen after synanche, but in parts which are adjacent to the ulcerations; and paralyses of the muscles of the pharynx, larynx, or of the eyes, may supervene only at a later period, or may be altogether absent.

You see, Gentlemen, that the paralyses observed after synanche are by no means without analogies, and that certain peculiarities which they alone appear to possess, occur in truth in other paralyses, or find an explanation in the peculiar manner in which the pre-existing disease was localised. Also the circumstance, which no doubt is favourable evidence of the specific nature of the disease, that the paralyses may sometimes have occurred after very slight forms of synanche with only a trifling affection of the mucous membrane, cannot be held to be determinative. For likewise other acute diseases occurring in a slight form may, as we have been expressly told, be followed by paralyses.* Moreover I may point out to you that frequently a severe affection of the mucous membrane occurring in synanche, a diphtheritic ulceration situated upon the posterior aspect of the soft palate, is overlooked, and that in such instances the opinion is easily formed that the case is only one of simple catarrh. If we add this further consideration, that a number of trustworthy observers have noticed the occurrence of paralysis even after affections of the throat which certainly had nothing to do with synanche, after abscess of the tonsil, for example,† you will grant that there is at present no more necessity to assume that the paralyses following synanche are of a specific kind, than that there is anything specific about the paralysis following typhus or small-pox. I think, therefore, that it is better not to be too quick in making assertions as to the specificity of these paralyses which would impede rather than assist further investigation, and that we ought to say that a disease is of a specific nature only when there is nothing analogous to it, or

* Vide Nothnagel, *l. c.* p. 508.

† On this subject see Senator in Virchow's Archiv lvi. p. 77.

when other conclusive proofs may be adduced. Such a proof would, in the case before us, be given by the observation that a patient attacked by paralysis during convalescence from synanche produced in others an attack or even an epidemic of synanche through this very paralysis; in other words, by the observation that such a paralysis was infectious. As far as I know, however, an instance of this kind has never yet been observed.

I now come to one of the most important points, and one, moreover, which has been very much discussed of late years, I refer to the causes and nature of synanche. As regards the general etiological factors, you know that synanche is pre-eminently a disease of childhood, and that it most frequently attacks children from their second to their seventh year, and also somewhat less frequently up to their twelfth year, the disease at these ages being especially dangerous from the fact that the larynx is prone to become involved. After the twelfth year, synanche, generally speaking, occurs more rarely, and it is only in exceptional cases that the disease becomes localised in the larynx. I have already mentioned that the disease is infectious, and that it possesses this character is proved not only by the facts that sometimes a number of persons are attacked together, but also in a very especial degree by instances of transmission from individual to individual, and by cases showing the latency of the disease. It appears unquestionable from these observations that the infectious material is attached to the mucus, pus, and shreds of tissue of the diseased portions of the throat. The attempts, however, to produce synanche in the human subject by inoculation have not hitherto been successful. Neither Trousseau nor Peter, who, with wonderful boldness, performed these experiments upon themselves,* was able to discover any results after transferring these morbid substances into the tissue of the skin and various mucous membranes. Experiments of this kind, which have been also made in other infectious diseases, scarlet fever and measles, for example, are by no means evidence against the infectious nature of synanche, but they only prove that we do not always possess the conditions necessary for transmission.

In recent times, however, the attempt has been made to

* Vide Trousseau Medic. Klinik übers. von Culman. 1866. i. p. 416.

approach the question with regard to the nature of the infectious material in another way, namely, by microscopic examination and by inoculating animals. The view which is the starting-point for these investigations, and which is neither unjustifiable nor improbable, is that the contagium of synanche, as of the other infectious diseases, may be a living organism, certainly of infinite minuteness, and invisible, at least to the naked eye. As a matter of fact, the microscope teaches us, as I have already incidentally told you, that in the mucus, in the deposits in and upon the affected oral and pharyngeal mucous membrane, there are found certain very minute organisms, most probably of a vegetable nature, which are now usually called micrococci, globular bacteria, and also monads. These are round or oval bodies with a distinct contour, about one-tenth or one-twentieth the size of a blood-corpuscle, which are found singly or in masses, and can scarcely be distinguished from the so-called granular detritus or disintegration-granules, or from the minutest albuminous and fatty molecules. That these bodies are of an organised and parasitic nature is concluded rather from the general effect, from their diffusion and development, than from the physical and chemical properties of the individual granules. These micrococci, which are to be found in the pharynx of patients suffering from synanche, resemble, at least in all their external characters, those fungoid elements which are found in putrescent fluids, and which are present also in the mouth and throat of perfectly healthy individuals, in the mucus between the teeth, or in the contents of carious teeth, as constant attendants upon the processes of decomposition which are going on. We find in putrefaction, in addition to the micrococci above described, other structures, rod-shaped (bacteria in the strict sense), necklace-like chains, and long jointed filaments (Mycothrix, Leptothrix). These, however, occur also in synanche, and there is no importance to be attached to the relative proportions in which they exist, or to the preponderance of one or the other form (which might, perhaps, cause a distinction to be made between the peculiar fungi of putrefaction and those organisms which occur in synanche), inasmuch as the relative amount of the individual forms varies very much in real putrefaction and depends upon the stage of the process, also upon the consistence or condition in other respects of the

putrescent substances, and upon other circumstances as well. Nevertheless the mere external resemblance between the micrococci and the organisms in putrefaction would not in itself justify us in regarding these two as identical; the former, however, might be only a lower stage of development of peculiar specific organisms having a causative relationship with synanche. In order to recognise and demonstrate them as such, the micrococci, which are supposed to be specific, must be isolated from the fluids and tissues of the body and freed from all admixtures, and allowed to develope themselves on a soil suitable for their growth until they have attained a thoroughly characteristic form, by which they may be distinguished with certainty from other organisms, and particularly from the fungi of putrefaction in their various stages of development. This is one of the most difficult of tasks, and one which can at the present time be scarcely fulfilled so as to meet all requirements. For how can we be certain that the micrococci have been so isolated from the diseased tissues that not a single trace of any foreign substance adheres to them? But even if we had succeeded, though only approximately, in doing this, we ought to be able by the breeding process, by the so-called pure cultivation, to obtain really characteristic organisms in various stages of development and with regard to which no mistake could be made. Unfortunately our present knowledge shows us that this is not the case. The study of the nature and development of the lowest organisms, which is the question in point, is a very obscure department of botany, in which, notwithstanding the numerous investigations by most celebrated observers, few positive results have been obtained, and even a still smaller number which can at present be turned to account for pathological purposes. For on this subject the sources of error are constantly accumulating. Hence it is that what one observer describes as a specific micrococcus, another refuses to recognise as such, and regards it only as a modification of the ordinary fungus of putrefaction, or as an artificial product. The difficulty, therefore, of finding one's way in this confusion of conflicting opinions is still further increased by the fact that in consequence of the various notions, which, as I told you at the commencement, are connected with the nomenclature of the disease, we are often completely in the dark as to the points upon which the disputes turn, and for

which this or that observer contends; whether a significance is to be attributed to the micrococci for the clinical features of the disease in all its forms, or only for definite anatomical symptoms; whether these latter alone are to be considered as characteristic of synanche, and if so, whether they are the consequence of specific or non-specific micrococci.*

I will endeavour to pick out for you, as distinctly as possible, the pith of the questions in dispute, and to indicate to you that standpoint which, in my opinion, after impartial examination of the facts, seems to be the most correct, at least for the present. In the first place, there is the question (and this even is an old point in dispute), whether the various anatomical forms above and below the vocal cords, especially in the one case, the slough-producing or diphtheritic inflammation, and in the other, the free, fibrinous (croupous) inflammation, are always, and, under all circumstances, etiologically connected; whether, therefore, they are always produced by one and the same cause, the contagium of synanche. Many adopt the view, that the croupous inflammation of the air-passages may by itself represent a special disease, without any participation of the structures of the throat, and independently of synanche. This view cannot be rejected as absolutely false, partly because in former epidemics, and up to the middle of this century, and in many countries sometimes even at the present day, there has been an absence of any affection of the structures of the pharynx from the beginning to the end of the disease; and partly because, after the positive statements of various investigators, it is impossible to doubt that a croupous inflammation of the air-passages may be produced by intense (chemical) irritants, which have nothing whatever to do with synanche. The demonstrative force of such proofs cannot be shaken by the negative results obtained by others. But in by far the great majority of all cases, at least at the present day, the croupous inflammation of the larynx and trachea is the result of the contagium of synanche, and occurs almost only in epidemics of this complaint, and

* For the reasons given above I must refrain from naming the various authorities for each view, and will simply refer to the works of Hallier, Hüter, Letzerich, Nassiloff, v. Recklinghausen, Classen, Oertel, Senator, Eberth, Leber, Stromeyer, Klebs, and Billroth.

together with its other anatomical forms. On this account, the general opinion now is, that "croup" and "diphtheritis" are only part-symptoms of one and the same disease, which we call synanche, an opinion to which we subscribe, under the above reservation.

The question further arises, whether the infectious material of synanche (in the sense in which we use this word), exists in the micrococci, which are found in certain products of the disease; and whether, therefore, all the anatomical changes, which we have learnt to recognise as the outward expression of the disease, are the direct or indirect effect of those organisms. Very closely connected with this is the other question, whether this effect belongs only to peculiar specific micrococci of synanche, or to others of a non-specific kind, particularly to the organisms found in putrefaction. With regard to this, we must keep firmly in mind, in the first place, that, as I have already said to you, up to the present time, in no way has it been possible, either by chemical or physical tests, or by cultivating the micrococci, to distinguish positively those occurring in synanche from similar organisms which make their appearance in some putrefactive changes, so that, on the contrary, their identity is expressly insisted upon by many.* We must also, in the second place, remember that, as experiments on animals have shown, infection with putrescent substances produces exactly the same primary and secondary anatomical changes as matter containing micrococci from the throat of synanche patients; indeed, that whole generations can be destroyed (Majendie,† Davaine‡) by successive inoculations with the blood obtained from an animal infected by the poison of putrefaction, therefore septicæmic, and that just the same holds good when an animal has been infected with the products of synanche (Oertel§). If, therefore, up to the present day, neither in their external characters, nor in their effects, has any difference between the micrococci of putre-

* Vide Hüter (Centralbl. für die med. Wiss. 1868, p. 547; Chirurgie von Billroth u. v. Pitha iii. p. 20. Allg. Chirurgie 1873. p. 360, etc.). Billroth Untersuchungen über die Vegetationsformen von Coccobacteria septica. 1874. pp. 91. und 179.

† L'Union Méd. 1852. No. 70 *et seq.*

‡ Bull. de l'Acad. de Méd. 1872. No. 31 *et seq.*

§ Deutsches Archiv für klin. Med. viii. p. 242 *et seq.*

faction and those of synanche been established, there appears to me to be nothing at present to justify the assumption that the latter are of a specific nature. The idea, therefore, might be well entertained that, after all, the micrococci of putrefaction are the cause of synanche, and play the same part in exciting the local and general symptoms of this disease as they are credited with—whether rightly or wrongly may here remain undecided—in putrid infection. We might, therefore, suppose, with reference to what has been just adduced, that the putrid decomposition continually occurring in the mouth and throat becomes increased from some cause or other; that perhaps an existing catarrh, or a trifling lesion of the mucous membrane, may favour the entrance of the putrefactive poison, especially also that of the micrococci; and that, in consequence thereof, different changes take place, according to the locality. Thus, for example, in the throat, where they penetrate more deeply, we find sloughing (diphtheritis); where, however, they remain superficially adherent, as in the larynx and trachea, there is the free, fibrinous inflammation (croup); and we may, in the last place, suppose that the general disorder is of a septic nature. Weighty arguments, however, may be adduced against this view, which is supported by Hüter, and also against the view which regards the micrococci that gain entrance from without as the first and only cause of synanche. Certainly, as you will remember, in no form of synanche do we find micrococci in the air-passages below the vocal cords in any considerable quantity, of course when putrefaction, or infection from other parts, is excluded. (See p. 412.) Then, the disease of the pharynx, of whatever kind it may be, very rarely indeed extends to the œsophagus, whilst we cannot understand why the micrococci should keep clear of the latter, and why their spread should be confined to the air-passages. But there is a clinical fact which especially controverts that view, and it is one which every physician can prove from his own experience, namely, that, in scarlatina, we find the regular occurrence of exactly the same anatomical changes of the structures of the pharynx as in synanche—the catarrh, the detachment of epithelial shreds, the diphtheritic inflammation, and gangrene; while, on the other hand, that the air-passages are very rarely affected, which is the opposite to what is found in synanche.

You will, Gentlemen, in your future practice, often enough be able to satisfy yourselves of the truth of the assertion that, in scarlet fever, nothing is more frequent than diphtheritis of the throat, and nothing more rare than croup of the air-passages. And yet there can be no greater resemblance between two local affections than that between the affections of the throat in scarlet fever and in synanche. The resemblance, indeed, is so complete, that many observers, regarding the anatomical appearances as the essential part of the disease, consider the diphtheritic form of synanche to be only a scarlet fever without the exanthem, but, in other respects, look upon both diseases as identical, which, by the way, they are as little justified in doing, as they would be in considering every case of acute nephritis as scarlatina sine exanthemate, only because the kidney affection is a regular or frequent attendant upon scarlet fever. The micrococci likewise are in the throat affections of scarlatina just as numerous, and just the same in character, as those in synanche. But if, notwithstanding this, the air-passages in the latter disease are in a manner so very frequently attacked, and in the former so very rarely, we must certainly admit that these organisms to be met with in the throat can, neither in the one disease nor in the other, be the primary and essential cause. The fact that both diseases dispose to similar disorders of the throat, but only the one to a disorder of the air-passages, must be due to the difference in their natures, to a cause at present unknown to us. With regard to disorder of the air-passages, synanche comes nearer to measles, which, in contrast to scarlet fever, disposes far more rarely to the throat affections, particularly the diphtheritic ones, but regularly leads to diseases of the air-passages, croupous inflammation being the most frequent form which these assume.*

Only in one respect, perhaps, do the micrococci play a part, in as far as they produce a definite form of inflammation—the diphtheritic, or that which leads to the formation of a slough, and in which synanche becomes localised, and also other diseases, scarlet fever, for example. What we mean is this,

* This controverts Letzerich's view (Virchow's Archiv liii. 493 *et seq.*) according to which the croupous inflammation of the air-passages is said to be, not the direct effect of the fungi, but a consequence of the irritation exerted by these in the parts above, and continued to the structures below.

that wherever there is a superficial sloughing, a diphtheritis, whether upon the mucous membrane or upon the integument, whether as a single and independent disease, or as an attendant symptom on a general disorder, we find, without exception, micrococci, which, as above said, are at present undistinguishable in every way from the organisms found in putrefaction in one or another stage. It is therefore natural to imagine that a causative connection exists, and the more so because, as appears to result from recent investigations on this subject, by transferring putrid matter containing micrococci to an inflamed spot, the inflammation may be made to assume a diphtheritic character. The objection that in such operations other injurious agents in addition to the micrococci are transmitted, at present quite unanswerable, has certainly not yet been overruled, whilst, however, it has been made very probable that those fungoid elements play an important part in diphtheritis of the skin and mucous membranes, and in this I entirely agree with Hüter in considering diphtheritis to be a wound-myco sis, due to the elements of putrefaction. For the activity of these elements to be developed certain favourable conditions must be present. In the first place these elements must be present in sufficient quantity, and, what is very important, but has not hitherto been particularly attended to, they must be in a stage of activity and capacity for development; and, in the second place, the tissues must not oppose any adequate resistance to their entrance, and still less to their gaining a footing and obtaining a settlement. You are well aware, Gentlemen, that the excitants of putrefaction, to use an ordinary expression, are everywhere present, but that it is only at certain spots—the so-called putrefactive foci—that putrescence very readily occurs, because these elements are here present not only always in great numbers, but partly also because they are always in their most active stage, as shown by their continuous development. In the mouth and throat, which doubtless form such a putrefactive focus, every inflammation is on that account from the very commencement in danger of becoming diphtheritic, especially as soon as the resistance of the mucous membrane to the entrance of the micrococci becomes diminished, its epithelium destroyed or relaxed. This is less likely to happen at those places which, like the dorsum of the tongue, are furnished with

a thicker layer, and, on the other hand, would be much more likely to occur in the pharynx, palate, gums, lips, and borders of the tongue. Here, indeed, every small ulcer, no matter from what cause, every aphthous ulceration, etc., easily becomes "diphtheritic." That, on the contrary, the mucous membrane of the air-passages from the vocal cords downwards is so little disposed to diphtheritic inflammation is probably due in part to the fact that, as Hüter likewise maintains, the movements of the ciliæ make it difficult for the micrococci to gain a footing and obtain a settlement, and partly also to the fact that the air in the bronchial tubes and trachea contains a large quantity of carbonic acid gas, which most certainly possesses antiseptic properties. In this way we may well explain the comparative rareness of putrid bronchitis, as contrasted with the excessive frequency of the simple, catarrhal form. Other mucous membranes have again other local means of defence; thus, for example, the conjunctiva is protected by the movements of the lids and the lachrymal secretion, the saline constituents of which are unfavourable for the development of the micrococci. In such cases a peculiar factor is requisite to favour the lodgment of the micrococci, and this is the retardation or stagnation of the current of the blood. In a healthy, vigorous state of the circulation, the micrococci, even if they have anywhere gained entrance into the tissues, may be easily carried off and removed from the system; whilst tissues whose circulation is continuously languid, or which are supplied with unhealthy blood, offer the most favourable soil for the settlement of these organisms. Therefore it is that wounds and inflammations of the skin, or of various mucous membranes, easily become diphtheritic in persons who, in a general way, have become reduced by severe febrile or constitutional disorders, by loss of blood, bad feeding, or from mental causes; and that the same thing occurs at spots where, in particular, disorders of circulation, stagnations, or perhaps laceration of vessels have taken place—pre-supposing always that the surrounding air is charged with micrococci.

Taking all these facts and considerations as our basis, I now think that we are justified in forming the following conception of synanche, and of its relationship to the various forms of inflammation. The unknown infectious material, the contagium of synanche, disposes to a violent inflammation of the

mucous membrane of the parts where the respiratory and alimentary canals cross each other, and of particular contiguous portions, just as scarlet fever predisposes to an inflammation of the mucous membrane of the throat, and measles to a similar affection of the larger air-passages, of the nose, and connective tissue of the eyes. In consequence of the inflammation, there is in the throat, except in very slight cases, not merely simple catarrh, but also great softening and detachment of the epithelium, and also small losses of substance, and the more or less denuded, sore mucous membrane is exposed to the entrance and lodgment of the fungoid elements present in the throat, the action of which causes death of the uppermost inflamed layers —sloughing or diphtheritis. The diphtheritic stage is therefore only somewhat of a secondary process, and may, just as in synanche, be added also to the inflammatory throat affections due to other causes, to those occurring in scarlet fever and in small-pox, and to syphilitic ulcerations, etc.; and it is the more likely to supervene, the more favourable the other conditions, *e.g.* the state of the circulation, are for the lodgment of the micrococci. In synanche, especially, such favourable factors are the chronic catarrh, with relaxed, swollen mucous membrane, the hypertrophied tonsils, which we find in scrofulous children, who are so frequently the subjects of synanche, and in whom, moreover, the blood is apt to become stagnant in those parts. In addition to this there is the disorder of circulation which is caused by the rapidly-formed and copious exudation in the tissue of the mucous membrane; and then, too, as the disease goes on, there is also the weakening of the heart's action produced by the fever, which latter is certainly not without influence upon the general strength.

If but once the diphtheritis has become developed in the throat, and a suitable nidus thus furnished for the micrococcus brood, and a more favourable one can scarcely be imagined, then, of course, an atmosphere previously free from these organisms may become infected, and every ulcer which is exposed to this atmosphere runs the risk of becoming diphtheritic. But with the transmission of the mycosis there is not necessarily also a transmission of the poison of synanche. The former can also be transmitted by the mycotic, that is, diphtheritic inflammation of the throat occurring in scarlet fever,

or other severe febrile affections, by the diphtheritic inflammation of the mucous membrane of the genital organs, by diphtheritic wounds of all kinds, and can in general originate under unfavourable conditions in every atmosphere charged with micrococci. It might well be imagined that also the contagium of synanche enters into the system in this way. This, indeed, is usually taken for granted, and in proof thereof cases are brought forward in which paralyses occurred after a wound-mycosis. I have already explained to you that at present I cannot admit that the paralyses are a specific test for deciding whether a previous disease had or had not been synanche. It certainly not so rarely happens that, after an extensive wound-mycosis, a wound-diphtheritis, which had absolutely nothing to do with synanche, there may remain a prostration lasting for weeks or months, possibly even a paralysis, although I cannot say I recollect a case of the kind.* The paralyses, therefore, which moreover in such cases occur exceedingly seldom, do not as yet prove the entrance of the synanche poison from wounds. Inasmuch as we employ the term synanche to designate only a certain disease running its course with affections of the mucous membrane of the throat, we must prove the occurrence of such a complaint after the infection of a wound before we could admit that synanche could originate from such a cause. We must, in this case, so long as we are not actually and certainly acquainted with the infectious material, regard the disease from the same standpoint as we adopt for the other infectious diseases. We apply, for example, the term typhoid fever (Ileotyphus) to designate exclusively a series of very definite clinical and anatomical symptoms. This set of symptoms is produced when certain materials containing the unknown poison are taken up by the intestinal canal; possibly also by the organs of respiration. These same materials, however, introduced into the body through wounds, do not, as far as I know, produce those symptoms which are held to be characteristic of typhoid, but others of a septicæmic kind, and on that account, the origination of typhoid

* In Billroth's work (p. 182) I find a case of this kind. An assistant of his had a small sore on his finger, and this was infected by a wound occurring after herniotomy, which had become diphtheritic. A state of prostration ensued and lasted for a long time, and by degrees assumed almost a pernicious character.

by the infection of wounds will be denied as long as no instance occurs of that very definite set of symptoms being produced in this way. You will, Gentlemen, justly ask me how the diphtheritis of mucous membranes and wounds, once set up by the fungoid elements, can, without any treatment, be arrested and cured, as undoubtedly very often happens, even in the diphtheritis of synanche, although the nidus, in which in this case the organisms have lodged, permits of their development *ad infinitum*. This, so natural a question, is scarcely, or only very slightly, alluded to by many investigators, who ascribe to the fungi a specific or non-specific action in the mycosis of wounds, and I can, therefore, only place before you my own personal view. I think that the micrococci, in cases where the death of the individual has not previously taken place, may possibly only penetrate the tissues to a certain depth, until they come in contact with a stronger current of blood, by which they are dispersed and removed from the body; possibly also at a certain depth, with thrombosis of the vessels and stagnation of the circulation, there may be an absence of the oxygen or other materials which these organisms require for their existence.

Before I proceed any further, I will once more remind you that the views here propounded are mainly based upon the following considerations. In the first place, that in scarlatina exactly the same affections of the mucous membrane of the throat are of regular occurrence, as in synanche, and are accompanied by, so far as our knowledge extends, precisely the same micrococci; while, on the other hand, the affections of the larynx and air-passages (croup in particular), so frequent in synanche, are only exceptional occurrences in scarlatina, and that, on the contrary, the croupous affections occur in measles often without diphtheritic or other throat disorders. Secondly, that the micrococci, which are found in synanche in the mouth and pharynx as low down as the vocal cords, cannot at present be in any way distinguished from the organisms discoverable in one or another stage of putrefaction. Thirdly, that the diphtheritic condition of wounds, and of inflammations of the skin or mucous membranes, is most probably due to the same organisms, the micrococci of putrefaction. I, therefore, by no means conceal from you the fact that these arguments are not

as yet firmly established. It may be that a difference between the micrococci which are regarded as identical may yet be demonstrated in a really convincing manner; or that the assumption that the diphtheritis of wounds is a mycosis may turn out to be incorrect; but it will be time enough, when all this has been done, to construct other theories with regard to the nature of synanche.

I must also enter somewhat minutely into the diagnosis of synanche, although this is not generally considered difficult. If, as is thought by many physicians, synanche includes all those, and only those, affections in which a membranous deposit takes place in or upon the mucous membrane, then there is scarcely any difficulty about the diagnosis, for this is generally completed as soon as the throat has been examined. But from the foregoing description it is evident, that by this view we should, on the one hand, include under synanche various affections of the pharynx which have no connection therewith, more than an external resemblance to some of its forms, and, on the other hand, we should completely pass over the simple catarrhal form, though it certainly belongs, etiologically, to synanche. If the poison of synanche could be demonstrated by means of the microscope or otherwise, its presence alone would be sufficient to establish the diagnosis. The external appearance of the mucous membrane would be of no consequence, or at least only when, having established the fact of the poison, it was necessary to decide upon the particular form of the synanche —whether it was catarrhal or diphtheritic, etc. Unfortunately we have not yet got so far as this, and therefore all that we can do is to depend upon certain external conditions (as is the case in many other infectious diseases, cholera, for example), which in any special case justify us in assuming that infection has taken place. The most important of these is the existence of an epidemic. Where many cases of disease have already occurred, especially among children, accompanied by the above-described symptoms of synanche, we shall be justified in concluding that every new and similar case is one of the same kind, especially if the patient has been proved to have come in contact with those previously affected. There can only be difficulties with regard to the first cases of an epidemic which has not yet been proved to exist, or where the cases are only

sporadic and single, just as it is difficult to diagnose cholera when occurring without an epidemic, because a confusion may easily arise between this disease and certain forms of poisonings, *e.g.* from arsenic, and from decaying articles of food, as well as between it and other affections resembling its slighter forms (choleraic diarrhœa, cholerine). Thus, indeed, mistakes may arise with regard to the slighter and non-fatal forms of synanche, the pharyngeal catarrh with or without detachment of epithelial shreds, the catarrh of the larynx and trachea, slight diphtheritic ulcerations on the tonsils, etc., all of which occur quite sporadically, or at the commencement of an epidemic. There is less liability to mistake when we meet with the more severe forms, in which all the symptoms are exhibited in their full perfection and typical course, and which, therefore, commence with fever and the affection of the pharynx, which is then followed by that of the larynx. In exceptional cases, where the larynx alone is affected, the pharynx remaining normal, or so long as the pharyngeal affection is absent, as in the so-called "croup ascendant," the case must certainly be set down as one of synanche, if an epidemic of that complaint be prevalent at the time; but in the absence of an epidemic, an absolutely certain diagnosis is scarcely possible. The dispute, whether there is a purely local croupous affection of the larynx, an idiopathic croup, not dependent upon the infection of synanche, or that of measles, as I, for my own part, hold to be conceivable (see p. 432), will last until we discover the contagium of synanche. Moreover, with regard to the larynx, the form and extent of the affection of the mucous membrane can, as I have already told you, only in rare cases be determined by examining the parts with the laryngoscope. Generally speaking, we may conclude that we have to deal with a more intense affection, that is, one which is not merely catarrhal, but a croupous and diphtheritic inflammation, when the pharynx also shows signs of more than a simple catarrh, and when the symptoms of stenosis are very violent, and last longer than one or two days. The swelling of the lymphatic glands of the neck is also mentioned as indicating a more intense form of disease, but this symptom is often of no service as a guide, because synanche is very prone to attack scrophulous children, in whom the glands are already swollen. The diagnosis is

quite certain only when membranous casts are coughed up from the larynx.

The presence of a real epidemic of synanche, which has been proved to be such, is also important for diagnostic purposes in cases in which some one of the anatomical changes occurring in synanche is added to some other febrile disease. If, therefore, as frequently occurs, a catarrhal or diphtheritic pharyngitis accompanies scarlatina or other severe affections, such as small-pox or pyæmia, or if measles is complicated with a croupous laryngo-tracheitis; this is no evidence that synanche has supervened upon these febrile complaints; and still less must we say that synanche is present when, in the course of such diseases, a diphtheritic inflammation attacks other mucous membranes—that of the vulva and vagina, for example. Diphtheritis occurs in these cases because the disease sets up a predisposition thereto, in like manner as typhoid predisposes to bed-sores. Do not forget that synanche and diphtheritic inflammation are not synonymous terms, and that the former may occur without the latter, and *vice versâ*. It is true that synanche may complicate other affections, and especially the acute infectious diseases, just as occurs with other affections of this class; typhus and recurrent fevers, or two acute exanthemata may be joined together and create mixed forms of disease. But the assumption of such a complication of synanche with other diseases would only be permissible when either a decided epidemic or, at least, the perfectly developed series of pharyngeal and laryngeal affections was present at the time. Moreover, between scarlet fever and synanche there exists a certain correlation, inasmuch as both epidemics frequently occur simultaneously, or follow each other in quick succession, as if each set up a predisposition to the other. It is well known that similar relationships exist between measles, whooping-cough, and epidemic mumps, and between typhus, recurrent, and intermittent fevers.

With reference, in the last place, to the hitherto so-called diphtheritic paralyses, these have, it is true, certain peculiarities, which I have already explained to you, and which are principally evinced by the manner in which the paralyses spread. From these peculiarities we are enabled to draw certain diagnostic conclusions, though not exactly in the way that is generally

supposed. If, therefore, in children especially, a paralysis commences in the muscles of the palate and pharynx, and from thence rapidly passes to the larynx, the muscles of the eyes, and perhaps to other parts beyond; if, at the same time, there is no evidence of a central, or, at least, of an intracranial lesion, you might then fairly assume that the paralysis was the consequence of an antecedent affection of the throat, probably febrile; and inasmuch as synanche is by far the most frequent of all affections of this kind in children, you might indirectly conclude that it, in all probability, had been the precursory disease. There might have been, however, though such a case is very rare, another antecedent and cause, an abscess of the tonsils, for example, or some disease of the pharynx not depending upon the infection of synanche.

After what I have told you with regard to the course and termination of synanche, it is unnecessary to say anything in particular as to the prognosis. I pass at once to the treatment, with regard to which there are just as many diversities of opinion as exist on so many points connected with the theory of this disease. Everybody agrees upon this point, that during an epidemic the patients should be isolated as much as possible, so as to prevent the spread of the disease. How this is to be carried out in individual cases cannot well be provided for by general directions, as so very much will depend upon the external circumstances and conditions of intercourse and habitation, just as in other infectious diseases of children. We are unacquainted with any measures of a strictly prophylactic kind; there are none, for example, which act as vaccination does with regard to small-pox. It is advisable, however, when the disease is prevalent, earnestly to warn persons against exposure to those injurious influences which produce catarrh of the pharynx, larynx, and air-passages, and to recommend frequent and careful cleansing of the mouth and throat, and the employment of gargles; at least for older children and adults. Our object in this is, on the one hand, to check any increase of the decomposition which occurs in these organs, and, on the other, to prevent, as far as possible, the danger of infection, the contagium being certainly conveyed through the air to the mouth and nose, and a diseased mucous membrane being a more favourable channel

or infection than a healthy one. Slightly astringent and antiseptic gargles, such as a solution of permanganate, or chlorate of potash, lime water, with the addition of tincture of myrrh, etc., are the most suitable remedies for the above purposes.

In the actual treatment of synanche, the primary indication unquestionably is to prevent the disease from spreading to the larynx and air-tubes; or, if our attempts in this direction be unsuccessful, to obviate the risk of suffocation which such an extension would produce, for, as you will remember, the fatal result is, in the majority of cases, due to suffocation. Unfortunately, both these indications can be only very incompletely fulfilled; indeed, for the former one we have, I believe, no means at our disposal. The affections of the larynx form a part of synanche, just as those of the pharynx and kidneys form a part of scarlet fever, or as inflammation of the respiratory organs is connected with measles; and all that we can do in all these cases, is, to use every means in our power to keep off all sources of excitement from the organs which are disposed to inflammation. When, therefore, the affection of the pharynx already exists, we must use the greatest care that all such sources of excitement, as catching cold, exposure to bad or dusty air, straining the larynx by much talking, screaming, and the like, are very sedulously avoided. I certainly cannot recommend the local treatment of the pharyngeal mucous membrane by strongly irrifant or caustic remedies, though these are much in favour among many physicians; in my opinion they are quite useless, and can only assist in causing the inflammation to spread to the larynx. An energetic treatment of the diseased parts of the throat can only be employed with one object, and that is, the removal of that cause which produces the disease of the air-passages. It is certainly possible that this cause, the poison, is first absorbed in the pharynx, that here its local effect becomes developed, and that it then spreads to the larynx. In this case assuredly, the treatment could not be sufficiently early directed to the pharynx, and could not be too energetic in character, supposing that, after all, the pharynx were a fitting place for local treatment of this kind. But what steps, we may ask, does the surgeon take, when he wishes to prevent the spread of a poison from an infected wound? He employs the strongest caustics,

those which have a deeply destructive action, or he applies the red-hot iron freely to the seat of disease. But this kind of treatment, which, at any rate, has a distinct meaning, is certainly quite out of the question for the pharynx, on account of the vicinity of large vessels and nerves, and of the brain itself. Cauterisations, however, as they were formerly, and are even now occasionally employed, and in which the membranous deposits in and upon the mucous membrane are destroyed by a stick of nitrate of silver or by chloride of iron, or some other superficially acting cauterant, can serve no good purpose whatever. I will not, indeed, lay stress upon the fact that the laryngeal affection, the true danger, may occur without any membranes or pseudo-membranes being present in the throat; or that these latter are often enough present in places where it is difficult, or even impossible, to see them, and even more difficult to cauterise them, as on the posterior aspect of the soft palate. But what kind of danger, I wonder, can such membranes in the throat produce? They are certainly not the disease itself, but the product thereof, and they form no mechanical impediment. The most that can be said of them is that, before their separation, they may become decomposed and putrid; but the effect which they may then produce, the septicæmia, only in exceedingly rare cases leads to fatal results. At the commencement of the disease, therefore, to use cauterants energetically and recklessly, to apply the red-hot iron, fuming nitric acid, chloride of zinc, and such like—all this is out of the question; to apply caustics, to produce coagulation here and there superficially—this is certainly useless. On the contrary, experience has shown that this latter plan is injurious, because it only increases the inflammatory irritation, and hastens its spread. Treatment by cauterisation has, therefore, and very justly, been almost universally abandoned, at least by us in Germany.

There is certainly another way of neutralising the infectious material, of opposing the setting-in and spread of the diphtheritic (mycotic) form of inflammation, and of preventing the gangrenous destruction of the tissues and its consequences; I refer to the employment of disinfecting remedies, but there are very many difficulties in applying these to the pharynx. We must certainly not imagine that any real disinfection,

such as is necessary for a disease which spreads so rapidly, can be produced by gargling the throat a few times daily, or by brushing it over with lime-water, or chlorine-water, or permanganate of potash-solution. How such disinfection is to be carried out we could again learn from the surgeon. What surgeon would nowadays brush over a few times daily with lime-water, etc., an ulceration already diphtheritic, or in danger of becoming so, instead of cleansing it most scrupulously in every nook and corner, thoroughly disinfecting it, and protecting it most carefully from the ingress of foul air? Could we entertain even the most distant idea of imitating this procedure, with any success, in the pharynx? If we attempted to disinfect the current of air constantly passing over the diseased tissues, either by chlorine or by hyposulphurous acid, or even by carbolic acid, we should, putting out of the question the discomfort of the applications, by these means produce irritation in the air-passages, which is just the thing which ought to be avoided at any price. It is impossible to arrange that there should be an uninterrupted flow of disinfectant materials over the surface of the pharynx, and the most that we could propose to do is to have the parts washed out, or brushed over, as often as possible, every few minutes, perhaps, day and night, with a strong disinfecting solution.

But, Gentlemen, this is not possible, even in adults, and older, intelligent children; we cannot all day long be incessantly worrying a patient, keeping him in a continual state of excitement and restlessness, and disturbing him in his sleep. All this would only make him very ill, and, finally, we must remember that, in the later years of childhood, and further on in life, the danger is by no means so great. It is only in younger children that the danger is very great. But, Gentlemen, just make the attempt once in your practice thoroughly to brush out the inflamed pharynx of a sick child, perhaps three or four years old, no matter what you use, it may be honey, and do this, not every two minutes, or every two hours, but only six or eight times a day. Just see how, besides yourself, it takes the father and mother, and others perhaps as well, to hold the refractory child (and even the best child becomes refractory the third or fourth time such a procedure is repeated) —see how it clenches its teeth with all its might, struggles and

kicks as hard as it can to get loose, cries and screams until it has literally screamed itself hoarse,—just experience all this once, and you will, like myself, when I still followed out this kind of treatment, not be able to resist the conviction that such a method as this is decidedly mischievous, that it is likely to hasten on the much-dreaded inflammation of the larynx, and that it greatly exhausts the strength of the child. It is much better to confess that a really thorough disinfection, such as is used for external ulcerations, and such alone can be of any use, is here impracticable. You may, moreover, be very sure of this, that, however strictly you order the throat to be brushed out so many times night and day, in the majority of cases your prescription will only be very incompletely carried out. I, for my part, have, on that account, for some years abstained from persisting in any such violent and energetic brushing out of the throat, and from attempting to do what really cannot be done. I content myself with advising that, as often as can be done without too great discomfort to the little patient, the throat should be washed out with a non-irritating disinfectant, preferably with permanganate of potash (1 to 300), or even with lime-water, or a solution of chlorate of potash, both of which, however, have only a very slight disinfecting action; or the same remedies may be applied by means of a soft piece of sponge, or a soft brush made of camels' or goats' hair. I do not imagine that any very efficacious disinfection is accomplished in this way, but I have the comfort of knowing that I am doing no mischief, and, at least, that I am doing as much, or it may be as little, good as those who recommend the strongest disinfectants, and fancy that that treatment can be really carried out. When the inflammation is very severe, if the mucous membrane of the pharynx is very red and swollen, and swallowing excessively painful, I order small pieces of ice to be put into the mouth as often as possible, and slowly sucked; and, as an internal remedy, I, like many other physicians, prescribe the chlorate of potash in a watery solution, and, according to the age, from a half to three decigrammes every two hours. This remedy has a not undeserved repute in all kinds of inflammatory and ulcerative affections of the mucous membrane of the mouth and throat,

and it may, therefore, always be used in synanche, as no better one is known.

When the pharynx alone is affected, I am but little disposed to advise any violent remedies, but when the larynx becomes involved, I decidedly think that we ought to adopt energetic measures; for we cannot do any harm, and may possibly do some good. I advise you always to commence the treatment by giving an emetic, even when the difficulty of breathing is not very great, and you are in doubt as to whether you have a simple catarrh, or a croupous-diphtheritic inflammation to deal with. The emetic always gives relief, which in catarrh frequently continues, and leads directly to recovery; in the other forms, certainly, the relief is only temporary, and depends upon the loosening and detachment of mucus, exudation, and shreds of tissue. I cannot say whether the other, not purely mechanical, effects of the vomiting, the excitement of perspiration, the diminution of the blood-pressure, have any effect upon the inflammatory process in the larynx; this is certainly possible, but the mechanical effects of the vomiting, and of the efforts which precede it, are certainly the main agents, and for this reason I cannot recommend you the otherwise very valuable apomorphia, which, moreover, in children is apt to produce collapse. In the choice of an emetic you must also take care that your remedy does not produce violent diarrhœa, or increase one that may be already present, for this would greatly assist in producing the much-dreaded collapse. It is therefore advisable to combine, in this case, very large doses of ipecacuanha, with excessively small ones of tartar emetic, or to avoid the latter altogether, and to give in its place the sulphate of zinc or of copper, which latter is no longer credited, as formerly, with any specific action upon the laryngeal affection. You must often repeat the emetics in the course of the disease, whenever the difficulty of breathing increases, or suffocation threatens to supervene.

Of the numerous remedies and methods recommended for the affections of the air-passages themselves, those unquestionably rank the highest which act directly upon the mucous membrane itself, and the greatest result is to be expected from inhalations, whilst the actual application of medicines, in substance or solu-

tion, is not to be thought of, because very difficult of accomplishment, and because we can never be certain that every part of the diseased tissues has been reached. The vapour of water is the simplest remedy for inhalation purposes, and it is one which you can, and should, first employ in every case. You require for this purpose no special apparatus; it is quite enough if you keep the air of the patient's room, and particularly the air around him, thoroughly moist, by having large basins placed about the room, and kept constantly full of hot water. The steam assists very much in dissolving the mucus, and in loosening and breaking up the membranous deposit. If the children are able, you may let them, from time to time, inhale the lukewarm vapour of water, camomile tea, etc., with any kind of inhalation-apparatus, the simplest being a pot over which a funnel is inverted. You may also use particular remedies in a pulverised state, or in the form of vapour, for the purposes of inhalation. We do this with the intention of loosening the membrane, and in this way removing the impediments to respiration. The caustic alkalies, however, which certainly possess the greatest solvent properties, are, as a matter of course, not suitable for our purpose, and therefore we have recourse to a few milder remedies, which certainly act much less decidedly, and more slowly, such as lime-water, solutions of nitre, lactic acid, and bromine. You must not expect much from any of these, for the amount which, during inhalation, reaches the larynx is by no means sufficient for dissolving the morbid products, and, in particular, the lime which is contained in the lime-water is rendered inefficacious by the carbonic acid of the expired air. The vapour of water is, in my opinion, always the principal thing in the inhalations; in addition to this, we employ a few substances, bromine especially, which have a strongly irritant action upon the mucous membrane, and induce expectoration, but their solvent powers are only extremely small. Lime-water is the mildest remedy, and is therefore borne the best; bromine, on the other hand, is the easiest to use, in the absence of any special apparatus. According to Schütz, a sponge should be saturated with a solution of bromine and bromide of potassium, of each .36 parts, in 120 parts of water, and placed in a cone of paper, and the patient should be made to inhale it for five or ten minutes, every hour or half-hour. You may, in

your future practice, first try any means you like; I can forewarn you that you will soon try another; and if this has not the desired effect, you will again have recourse to a fresh one.

In addition to the inhalations, there are a few external remedies, which I think ought not to be neglected, although their action is very uncertain, for, in such desperate cases, everything should be tried which offers the remotest chance of success. In vigorous children, when the larynx is severely involved, and especially when the fever is high, benefit is sometimes obtained from the application of mercurial ointment to the sides of the neck; this should be rubbed-in every two or three hours, and continued for two, or at most three, days. In the intervals between these frictions, I apply wet compresses to the throat, or I have it enveloped by a fat, broad piece of bacon-rind. Here, in Berlin, this latter is a very favourite and a very convenient domestic remedy; it acts in some measure like a plaster, as an impermeable coating, and, at the same time, it has a slightly stimulant effect upon the skin, and acts as a derivative. These two latter remedies, the wet compress or the piece of bacon-rind, may be tried by themselves in cases where there is any objection to the mercurial ointment.

The various methods and remedies which I have now mentioned comprise the whole of those which I have employed up to the present time; I am, however, far from promising any very brilliant curative results, or from advising you against a trial of any other means and remedies which are, or may be, recommended. Whatever treatment you may adopt, you will, in the great majority of cases, at last find yourselves met by the question as to whether or not tracheotomy is to be performed for the relief of the impending suffocation; or, rather, the question will be not merely this, but as to when the operation is to be done, and whether the time has arrived for its performance. For the operation is indicated in every case where the larynx is blocked up by membranous or pseudo-membranous formations; under such circumstances, we can scarcely ever anticipate that the disease will subside in a few days, and before fatal suffocation occurs, and also because the condition of the patient is always improved, rather than aggravated, by the actual operation, provided that this is not performed in the death-agony, and that no particular accident occurs. Tracheotomy is performed

in synanche with the especial object of gaining time, and it is therefore, strictly speaking, always indicated whether the impediment to respiration exists only in the larynx, or whether it also extends further down, or whether secondary changes have already taken place in the lungs. In the two latter cases, the chances of recovery are generally very much less, irrespective of whether tracheotomy is performed or not. We are not justified in saying, as is often done, that, under such circumstances, "the chances of tracheotomy are more unfavourable," because this might often be an inducement for its non-performance. The operation of tracheotomy makes no change in the prospects, or at least makes them no worse; but, when it is done at an early stage, and the after-treatment is carefully conducted, it may have the advantage of preventing those secondary changes from taking place. The physician, therefore, who superintends the treatment should consider that the moment for the operation has arrived, when the existence of a more than merely catarrhal affection of the larynx is indicated by great difficulty of breathing, continuing perhaps more than two or three days, or by attacks of suffocation, or other symptoms of the kind. In other respects, there are various circumstances connected with each individual case which aid us in determining the period for the operation, the strength of the patient being an important point; and, as a general rule in practice, the parents and friends, before they will give their consent to the operation, have to be convinced, by the distressing condition of the little patient, that there is no possibility of relief in any other way. It will not be easy for you, Gentlemen, to perform tracheotomy "too early" in the laryngeal affections of synanche, for it is doubtful whether the operation can ever be done too soon. For the details of the operation itself, and particularly of the subsequent treatment, on which alone the result often depends, I must refer you to your lectures on surgery, and your hospital experience.

Other indications than those just described can rarely be fulfilled in the course of synanche. The fever seldom calls for any medicinal treatment; the threatening collapse more often requires attention; but you will have learnt how to combat this from previous lectures. In like manner, you will know how to treat the debility and anæmia, the after-consequences of the

disease. The paralyses yield, as a rule, to tonic treatment, but the employment of electricity, and particularly of the induction current, produces a more rapid recovery.

Finally, we have no means of preventing the sudden death arising from fatty degeneration of the heart, or paralysis of the nerve-centres. The time which elapses between the advent of the threatening symptoms and their termination should be employed in administering the most powerful excitant remedies that you have at your command. You may use musk and wine, apply strong stimulants to the skin, as well as have recourse to the subcutaneous injection of ether, camphor, and similar remedies.

ON THE

# MILDER FORMS OF ABDOMINAL TYPHUS.*

BY

PROF. DR. JÜRGENSEN.

TÜBINGEN.

THE mild forms of the acute infectious diseases—with the single exception of those running their course with eruptions—have been much less clearly recognised by physicians than they deserve.

There are several reasons for this:—

The sharply defined forms of disease of compendious works—photographs necessarily re-touched to excess—are, for many observers, not diagrammatic drawings. They are forms, rigid forms, into which what is observed at the bedside must be made to fit. If it does not so fit in, so much the worse for the patient. For the most part, a treatment uncertain as to its objects, and therefore wanting in decision, will not tend to lessen the patient's sufferings.

Not unfrequently, and this just as influential a reason, symptoms of an acute infection but slightly developed are regarded by laymen as "rheumatic," "catarrhal" conditions, etc. The domestic remedy is here substituted for the physician.

Further, the diagnosis of many abortive forms can only be an *ætiological* one, and cannot be deduced from the given phenomena alone. But to be able to form ætiological diagnoses, one must see many patients. Any one who knows how much

* The terms "abdominal typhus" and "typhus" are used by German writers for the form of disease called "typhoid" by English writers. —Translator's note.

time and labour are required of the practitioner in a somewhat extensive epidemic will not be surprised that slight attacks are condemned to play the part of Cinderella. There are limits, purely physical limits, to the powers of even the most assiduous physician.

Cases of little severity are seldom brought into the hospitals.

There are thus reasons enough to render it intelligible that slight infections are less known. And yet it is equally as important for science as for practice that we should know them. I believe the proposition to be fully justified, *that the whole theory of the acute infectious diseases, as propounded at the present day, is untenable if the existence of the milder forms cannot be shown.* In other words, if an attack of disease is to be regarded as arising from infection, transitions from the most slightly developed to the severest forms must be demonstrated. *The connecting link for them all is their origin in a common cause.* Into this proposition I wish now to enter more at large.

We teach that for the occurrence of every case of infectious disease a something is necessary which is possessed of properties peculiar to itself, and not occurring anywhere else in nature in this same combination. This something, entering into reciprocal action with the human organism in a manner yet unknown, calls forth an anatomically as well as functionally sharply characterised alteration in individual organs and in the whole changes of tissue.

To describe the state of things still more precisely:—

Certain of the deleterious entities have the power of so changing the organism attacked by them that through it or out of it other similar organisms undergo the same changes. To these belong indisputably the acute exanthematous diseases. Other excitors of disease require, besides the body taken possession of by them, further conditions to enable them to extend their influence to other organisms of the same kind—typhus abdominalis, cholera Asiatica. A third class, malarious diseases, require only such factors as are present in nature outside the human body to enable them to develop themselves. In this statement of the case, justice has been done to the ancient, not in themselves very sharply defined notions, "contagium" and "miasma," without any distortion of the facts. And, above all, nothing has been prejudged to favour a systematic conception.

To render a disease possible, however, it is necessary that the exciting cause should encounter a body with which it can enter into reciprocal action. The disease resulting from the conjunction of these two factors will depend, for the *form of its appearance*, upon the intensity of the exciting cause and upon the power of resistance of the organism attacked. Both may vary within the widest limits. With the very great number of persons attacked every year, however, it is scarcely to be conceived that the highest as well as the lowest steps of the form of appearance of the disease should not come under observation.

If, on the one hand, this view offers generally a theoretical comprehension of what occurs, it requires of us, on the other hand, that we should find mild forms of all diseases arising from the assumed action of infectious agencies. And yet it possesses an indirect advantage. Any one who has accustomed himself to see in the disease the product of two factors, of which the organism of the patient and its power of resistance is one, will not easily forget that he has a patient to treat, not a disease.

I have so often observed that the recognition of the mild forms of acute infections has more and more escaped the attention of physicians, that I feel justified, before giving my own special view, in bringing under consideration more in detail the whole subject generally.

The action of two bodies upon each other is, *cæteris paribus*, dependent upon their volume. Our present object is to put this general formula, derived from experience, into practice. We have to inquire whether, in the theory of the acute infections, the assumption of simple *quantitative* differences of the infecting materials does not suffice to furnish a comprehension of the lesser or greater extension of the diseases occasioned by them, and of the greater or less severity of the individual attacks of disease.

The great variation in the intensity as well as in the extension of epidemics must first be mentioned here. Generally speaking, it may be said that the most extensive epidemics have, at the same time, also been the most intense.

Or, to take individual epidemics, it is when they are at their height that the most numerous and severest cases occur. Is not

this explained most simply by assuming that the presence of a large quantity of the poison determines the occurrence of numerous and severe attacks?

Before we can bring in analogically the rapidly fatal substances of the mineral and vegetable kingdoms denominated "poisons," there is one objection to be removed. That, in the case of poisons, the severity of the illness depends upon the quantity thereof which has come into operation, no one will deny. But, it may be said, in the case of zymotic diseases, we are forced to assume that a reproduction of the substance taken up, an increase of the quantity of it, takes place in the body, whence the quantity first introduced is more indifferent. This objection is less valid than it appears. We have direct counter-proofs.

No one can doubt that the poison of soft chancre also must reproduce itself before it can become active. But if we dilute this poison with an increasing quantity of an indifferent substance (serum), the period of incubation becomes prolonged with slighter dilutions, the consequent ulcer smaller, and, with a greater dilution with serum, always smaller and smaller. Finally, a pustule appears which we cannot with certainty designate as chancre. These experiments, originally performed by Köbner, have been verified by a long series of experiments performed by me. Here the *quantity* alone of the poison is changed.

Why does it happen that, with the introduction of a small quantity of the chancre-germs, no more extensive production thereof takes place? The answer to this question appears to me to be that the pus of chancre also is affected by changes of tissue. If, therefore, but few germs are introduced at one time into the organism, an accumulation of the chancre poison sufficiently great to destroy the tissues can never occur, on account of the nullifying power of the changes of tissue.

This runs parallel with the fact strongly insisted on by Ludimar Hermann, that the activity of a poison depends upon its concentration in the blood. But this concentration decreases continuously by the secretions from the blood.

Thus we see that the analogy between poisons and the matters which produce infectious diseases is an admissible one, and this analogy is decidedly favourable to the quantitative

hypothesis. There are many other things which may be adduced in support of it.

One of these is the dependence of abdominal typhus upon the level of the bottom water. After the subsidence of the bottom water, the most favourable conditions for the exuberant growth of the typhoid germ are present; the latter will therefore appear in great quantity after the lapse of a comparatively short time. According to our assumption, the conditions for a great extension of the disease would thus be given. The formation of centres of disease, also, as they occur in typhus, small-pox, measles, etc., from crowding of patients together, is easily explainable if we accept the quantitative hypothesis. There are cases, however, in which we are equally justified in assuming that a change takes place in the quality of the poison.

Thus with regard to vaccination. I am of opinion that vaccine lymph transferred from one human being to another for several generations, loses some of its power of infection. The following experiment appears to me to speak very decidedly in favour of this view: Six children of mothers who, while pregnant with them, had had small-pox, were several times vaccinated, without effect, from arm to arm, with ordinary lymph already long humanised. The vaccinations remained without result in all. Then, as soon as the negative result was fully determined, I had them all vaccinated with humanised lymph of the *first* generation. The most perfect pustules were formed in them all, after the first employment of this lymph. I have also made analogous experiments with a similar result here in Tübingen.

Now, why does the vaccine matter, which has grown so long on human soil, lose some of its power? We may picture to ourselves, that germ after germ may have become lost on the less favourable substratum, until all disappeared. This would represent the quantitative hypothesis. Or it may be assumed that the number of the germs has remained the same, but that their properties have become changed. Similar experiences are met with in other fields. This view attracts me less personally, but it is, at all events, possibly correct.

Now, whether we assume that it is the quality or the quantity of the poison which becomes changed, it is, at all events, admitted that a greater or less quantity of the active principle

may come into operation. This alone would suffice to enable us to understand the possibility of mild forms of disease, even if we assumed the susceptibility of the persons attacked, and their power of resistance against the poison taken up, to be always the same. But is this so? Experience contradicts it most distinctly.

I consider it necessary to distinguish between *power of resistance* and *susceptibility*. The poison of chancre does not take effect through the uninjured epidermis, or the uninjured mucous membrane. A man whose penis is furnished with a tough skin, will be less susceptible for this infection than one with a thin skin; but whether the latter, on absorbing an equal quantity of the active poison, will have an ulcer spreading further than the former, is by no means proved. Here the power of resistance of the tissues is the determining element. To avoid prejudging anything, we must, as it appears to me, distinguish between these two things, even when the infecting element is less known to us.

Whence comes it, then, that an individual, the circumstances of whose nutrition can be shown not to have varied, frequently becomes attacked at the end of an epidemic which he had resisted when at its height? The assumption here lies near at hand, that some obstacle to the admission of the poison has been removed, analogous to the epidermis or mucous membrane of the genital organs. Under such circumstances only, then, can the infecting agent come into activity. How otherwise could we interpret the "predisposing" errors of diet in connection with cholera?

It is just in the case of poisonings, in the more restricted sense of the word, that the power of resistance of an individual has been shown capable of variation between the most extreme points. To render this more clear, we must again distinguish between two things, the *power of resistance generally*, and the *power of resistance to individual poisonous substances*. The former coincides with what is usually called a good constitution. We can only gain a distinct idea of the nature of such a constitution by analogies. The axle of a railway carriage gradually becomes crystallised, and loses carrying power, in consequence of continued vibrations. So also may the joints of an originally iron constitution gradually become loosened by the cares of

life. Who can tell, by looking at two axles made in the same manufactory, which will last the longest? Even so little are we capable of judging *à priori* of the toughness of a human body. In the one case, as in the other, testing only can decide. Fortunate is the patient whose physician keeps his eyes open in that direction.

The power of resistance to poisons acting specifically varies immensely in different individuals. We observe many examples of this daily in life. Who does not know the effect of alcohol? One man drinks it to excess with impunity, another loses his senses after taking a small quantity only. We often observe something similar in the therapeutic employment of the alkaloids, especially of those contained in opium. In acute infections also something of the same kind occurs. Any one who has already had scarlet fever may generally expose himself to the poison with impunity; he is no longer susceptible for it, *i.e.* his power of resistance to that poison is very great. I know a person who has had scarlet fever *four* times in her life. The two last times she was attacked at a moment when only isolated cases were occurring, not during the height of the epidemic. Small-pox, measles, and abdominal typhus, furnish frequent opportunity for such observations. In such cases, the power of resistance to the poisonous entity which produces the disease has not been rendered greater by the previous attack or attacks.

And now for the reverse of the medal. How frequent, just in the case of scarlet fever, is the immunity from the poison; and there is probably no disease which does not constantly spare a certain number of persons. What would otherwise have become of the human race at the time of the prevalence of the plague?

If we regard all the points just mentioned as established, we must, I think, admit that, by their coincidence, the value of the individual factors being admitted to be a varying one, results may be attained which may range from zero to the maximum. The instantaneously fatal form of the acute infections, like the mildest, scarcely recognised as disease, fits into the frame.

A physician in actual practice, who has once become accustomed to take notice of mild infectious diseases, will very soon learn to appreciate their practical importance. This will be

the case especially with the disease with which we are now occupied, viz. *abdominal typhus*. It is not indifferent for the patient whether he is treated as one suffering from gastric catarrh, or whether his medical adviser knows that he has been attacked by an infection, slight indeed in its present form, but yet capable of endangering life.

The prophylaxis also for others is one to be carried out from the first moment in which we become aware that something exists against which we must adopt precautionary measures. For the sake of a "catarrh of the stomach" we do not employ disinfectants. Griesinger was the first who correctly appreciated typhus levissimus. He justly insists upon the fact that THE SHORT DURATION OF THE DISEASE, not the slightness of the symptoms in an attack of typhoid lasting for a normal time, is the determining sign. A doubt concerning that which Griesinger has observed has not, so far as I am aware, been uttered; but even so little has it been accepted on any side. I am in a position to bring forward facts which render the existence of typhus levissimus certain for any one who can and will see.

In July, 1865, about twenty of the servants upon the estate Knoop, situated in the vicinity of Kiel, males as well as females, were taken ill in the course of a few successive days. Six of these went into hospital, and had moderately severe typhoid, characterised by all the usual symptoms. One had hæmorrhage from the bowels; the others, who had at first exactly the same symptoms as those attacked more severely, recovered rapidly, but continued strikingly weak, and unwilling to work for some time. An investigation made on the spot by the physician who treated them, Dr. G. Weber, and myself, furnished no distinct link for the mode of the infection. All that could be ascertained was, that they had all been attacked about the same time, although their occupations had called them to points of the large and scattered farm-buildings very distant from each other. The symptoms in them all had consisted in headache, languor, frequent shiverings, loss of appetite, and rumbling in the bowels, with more or less violent diarrhœa. I must observe here that typhoid was extremely prevalent in 1865. In 1866 a similar epidemic occurred on the estate Hagen, also near Kiel. The physician who treated those cases, Dr. Nissen, in Schönberg, gave me an account of

the epidemic which confirmed in all respects what I have described above. Single severe cases of that epidemic had also come under my observation in the medical clinique, but I was unable to catch any of the slight cases *in flagranti*. An epidemic which developed itself in the year 1868, in the hospital of the University itself, furnished me however with the wished-for opportunity. Twenty-nine persons were attacked with typhoid within a short time, a few amongst the patients in the wards, the greater number persons belonging to the establishment. In a case which terminated fatally, the existence of typhoid was shown anatomically.

The picture was always exactly the same: febrile symptoms, with disproportionately great languor not corresponding to the temperature, and dyspepsia. The characteristic signs of typhoid very soon presented themselves, however: rose-spots, diarrhœa, enlargement of the spleen, and sometimes bronchial catarrh. The recovery was always comparatively slow.

But we cannot do without a *résumé* here. I select a very few cases only out of the large number (above 100) of my observations.

A case of four days' duration:

A man æt. 19, ill three days with violent shivering and consequent heat, headache, want of appetite, and 4—6 loose motions in twenty-four hours. On account of general languor, the patient had kept his bed on the day before admission. On the fourth *day of the disease*, the patient complains of headache, is tottering and somewhat confused. The *tongue* is dry, red at the edges, and covered with white fur in the middle. There are a few *rose-spots;* the *ileo-cæcal neighbourhood* is very tender on pressure; the *spleen* is not perceptibly enlarged; there is no *bronchial catarrh*. *Pulse*, 112; *temperature*, 102.5°. The examination was made at 2 p.m. The patient had a general cold bath; the temperature fell immediately, the confusion ceased, the heat of the body did not again exceed the normal, and the patient left his bed on the third day, and the hospital on the fifth.

This case was not one of those of the hospital epidemic. For the diagnosis, apart from the general points to be mentioned below, I call your attention to the great amount of confusion and, very especially, to the unmistakeable rose-spots.

It is remarkable that just in the cases of short duration, brain symptoms are more frequently strongly marked if no considerable increase of the temperature of the body occurs.

From the *hospital epidemic* I give further one case of six days', and one of seven days', duration.

*Maidservant, æt.* 23. Commencement of attack with slight shivering and headache. On the third day of the disease we found: rare *rose-spots,* some *enlargement of spleen,* no *diarrhœa.* The usual febrile and dyspeptic symptoms were present. The temperature 103.6° (examination at 8 p.m.). During the next days there was continuous fever with distinct evening exacerbations, which went up to 103.6°. In the morning the minimum was 100.2°—100.5°; on the sixth day, 103.6° in the evening; then rapid falling of the temperature of the body, which, on the seventh day, on an average of five measurements, did not exceed 100°.

*House-porter, æt.* 21. First symptom, sudden headache. *Admission* almost exactly twenty-four hours later. Unmistakeable enlargement of the *spleen,* pain upon pressure in the *ileo-cæcal neighbourhood.* Violent febrile symptoms with a temperature of 104.6°.

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| Morning temp. | 103° | ; | Evening temp. | 105° | on | 2nd | day. |
| „ „ | 102° | | „ „ | 104.8° | „ | 3rd | „ |
| „ „ | 101° | | „ „ | 102.5° | „ | 4th | „ |
| „ „ | 99.5° | | „ „ | 102.2° | „ | 5th | „ |
| „ „ | 99.2° | | „ „ | 102° | „ | 6th | „ |
| „ „ | 98.5° | | „ „ | 100.8° | „ | 7th | „ |

On the tenth day subnormal temperatures—97.5°.

I have confined myself to the very imperfect report of the cases, except as regards the taking of the temperature every two hours. During the very extensive epidemic of that time, very slight reports were made of the milder cases. My memory seems to tell me, however, that just in the cases occurring in the hospital epidemic the other symptoms of typhoid, rose-spots, diarrhœa, and bronchial catarrh, were almost always present.

The febrile movements were certainly shown to exist, and here we observe a deviation plainly enough from what occurs in the fully developed process of the disease.

Let us treat first of all of the *chief symptom* of abdominal typhus—I mean the *fever*. The commencement of the attack is to be dated from the moment at which the fever sets in; if shivering or even rigors occurred, then that marks the beginning. Every observer will agree with this view of Griesinger's fully and without reservation. It is only to be regretted that this important symptom for our calculation of periods is so often wanting in cases of moderately severe and severe typhoid. How does it stand in this respect with the less developed forms? I have accurate reports of 87 cases.

| Of these there were ended: | There began amongst them | | |
|---|---|---|---|
| | Suddenly. | With shivering. | Gradually. |
| On 4th to 7th day, 16. | All .. | 9 .. | None. |
| „ 8th „ 10th „ 19. | All .. | 11 .. | None. |
| „ 11th „ 13th „ 24. | 20 .. | 11 .. | 4 |
| „ 14th „ 16th „ 28. | 19 .. | 11 .. | 9 |

NEARLY HALF THE NUMBER OF CASES, THEREFORE, BEGAN WITH COLD; generally, a sharp rigor; more rarely, one or more shiverings. It was *suddenly, i.e.* so that the patient can distinctly name the day on which his illness began, THAT MILD TYPHOID COMMENCED 74 TIMES OUT OF 87 CASES; THEREFORE IN 85 PER CENT. No slight importance is to be attached to the circumtance that in the 35 cases, with a duration up to 10 days inclusively, a gradual course was not recorded once. The transition from the usual state of health to the fully developed disease occurs rather, as it appears, quite gradually in the attacks lasting from 11—16 days.

*A first peculiarity of typhus levissimus is, therefore, its sudden commencement.*

How does it stand with regard to the temperature of the body? Can the three stages first recognised as characteristic by Wunderlich, the initial period, the height, and the period of recovery, be demonstrated?

The *initial period*, characterised in fully developed typhoid by the graduated increase of *the temperature* during from 3—5 days, when an increase from morning until evening of about one degree, and from evening until the morning of the next day a decrease of about half a degree occurs, is almost entirely wanting. The hospital epidemic furnished me with a sufficient

number of observations which prove this. The great majority of these, it is true, refer to the second day of the disease. The increase of the temperature of the body occurs *rapidly*. The mercury has frequently gone up to 104° thirty-four hours after the first rigor. This corresponds to what was mentioned before, viz. that the commencement of the illness is distinctly recognised by the patient.

During the *height of the disease* the temperature of the body varies greatly. In the great majority of the cases—not excepting those lasting a very short time only—*the usual picture of the well-marked form* is to be observed—oscillations from morning to evening of 1—1½ degree, in which the *maximum* occurs in the *evening hours*, the *minimum* at a *late morning hour*. Deviations are, however, to be observed here also. First of all, *the comparatively great effect and the long duration of the abstractions of heat, and of quinine.* The lower temperatures last long, and the figures attained by them often approach very nearly the norm, and not unfrequently do so completely. The same holds good for the therapeutic influence of quinine. It is only a consequence of this fact that, after such effects upon the curve of temperature, great irregularities in the same frequently appear; that higher temperatures, for instance, are observed in the morning than in the evening. The same thing occurs not unfrequently, as is well known, in fully developed cases of typhoid if they are consistently treated antipyretically.

Side by side with the normal course of typhoid there is another which varies therefrom in height as well as in the opposite direction. We see comparatively often a form of *febris continua* which presents slighter remissions with very high temperatures. I have watched a case in which the temperature at 8 p.m. on the third day of the disease was 107°, and two hours later, in spite of the energetic employment of cold, 106°. Until the seventh day, the temperatures continued persistently over 104°, and only on the eighth day were lower figures, oscillating about 102°, recorded. On the tenth day the normal temperature was reached, and did not vary again. The diagnosis in this case was free from any obscurity—enlargement of the spleen, rose-spots, diarrhœa, slight bronchial catarrh, sufficed to form the symptomatic diagnosis.

In other cases comparatively low temperatures existed. In

one case observed from the commencement of the second day, the temperature on that day only once reached 104°; then on the whole of the third day only twice reached 102.2°, and, after fifteen grains of quinine, remained for almost twenty-four hours under 100.4°. On the evening of the sixth day of the disease, the temperature again went above 104°. On the eighth day the fever had entirely disappeared. Sometimes I have seen the temperature scarcely exceed 102°. These cases, into which I shall enter more minutely further on, are perhaps, practically, the most important of all.

The *third stage*—that of recovery—characteristic on account of its steep *curves: in the morning*, subnormal or normal temperature; *in the evening*, about 104°—is almost entirely wanting in the milder forms of typhoid; in any case, it is so slightly marked that we must have the wish to see it to be able to do so. The temperature generally falls gradually, to the same degree morning and evening, until it reaches the norm in 24—72 hours. I have never seen a return to normal temperatures completed in a shorter time than twenty-four hours.

The absence of the steep curves corresponds completely with what we know of their import. That, after the completion of the true typhoid process, the anatomical changes produced by it heal, is shown by them. In slightly developed typhoid, the steep curves are wanting which, in the highly developed form, for a long time control the issue of the whole process. Therefore, slight typhoid produces few anatomical changes—a fact which, if we take the duration of the disease as a standard, has an anatomical foundation.

I must remark moreover that this my last assumption concerning the absence of steep curves only holds good for mild typhoid treated dietetically and, as concerns the fever, properly. I do not doubt for a moment that the slightest infection may, by means of thoroughly injudicious treatment, be converted into a very severe one. Of this I possess proofs. I will only remark incidentally here that the duration of the steep curves in fully developed typhoid also, when it is properly treated, is very much shortened. I called attention to this fact years ago, and it has been verified by the most careful observers. (Immermann, Ziemssen.)

In my opinion, the reaction of the organism against the poison is clearly indicated in typhus levissimus by the state of the temperature of the body. On another occasion I have developed more fully my view of this subject.* I have pointed out that the typhoid temperature is compounded of the normal temperature, and of a surplus furnished by the disease. Each of the two concurrent factors has its share in the typhoid curve: the typhoid agent causes the increase of about 12.6°, the norm seeks to keep the temperature at about 98.8°. We have here insufficient infection in comparison with ordinary typhoid. The first-named factor comes more feebly into action. To express it quite generally, that factor is not able to impress its stamp upon the course of the temperature. *Why* it comes more feebly into action may depend upon various causes: the infection may be insufficient quantitatively, or the power of resistance of the organism may have been too great. I am inclined to attach most importance to the latter element, for the sudden commencement of the disease appears to me rather to show that a large quantity of the poison has been introduced. The insufficiency of this large quantity of the poison would be most simply explained thereby, that the typhoid agent was incapable of reproducing itself in sufficient quantity within the organism attacked. I will not spin out this hypothesis further, but I believe that every detail may be explained simply from this point of view.

To sum up, I say:

*The fever curve of typhus levissimus shows the insufficient infection in all the stages of the disease very especially thereby that it indicates the preponderance of the law which regulates the norm over the increase produced by the typhoid agent.*

How does the spleen behave? I can give information on this point also which is founded on exact observations.

*Enlargement of the spleen occurs probably in all cases, and is a constant symptom of even the mildest typhoid.* Out of 88 cases, in 7 only was an enlargement of the spleen not recorded as being appreciable. It was recognised, therefore, in 92 per

* The temperature of the body in health. Leipzig, Vogel, 1873. Chap. viii.

cent. The absence of the phenomenon is met with as often in the milder cases as in the more severe ones. Every one, however, who is familiar with percussion of the spleen, knows how easily a swelling of that organ, even if considerable, may escape detection. In typhoid, moreover, the tympanitic swelling of the abdomen renders the examination considerably more difficult. Here, also, there is a difference from ordinary typhoid: the enlargement of the spleen is often demonstrable *very early*, not very rarely on the second or third day of the disease. The increase in volume of the organ is frequently very considerable. The subsidence takes place rapidly.

*Rose-spots* also show themselves in mild typhoid. Their appearance is regulated, however, by the severity of the disease, measured by its duration. I will divide my cases into two classes.

I. Duration of the disease to the tenth day, with exanthem in 46 per cent. of the cases.

II. Duration from eleventh to sixteenth day, with exanthem in 75 per cent.

The exanthem differs greatly in extent, being generally greater when the disease is more severe. It appears early in mild typhoid; once already on the second day after the initial rigor; more frequently on the fourth or fifth day of the disease.

*Miliaria alba* occurs not very unfrequently, since many patients perspire violently. Once I have seen, on the second day of the disease, an erythema confined to the abdomen, somewhat resembling the change in the skin in scarlet fever, which disappeared rapidly again.

*Bronchial catarrh* is, with very few exceptions, but slightly developed. Altogether, it was recognisable in 28 per cent. of the cases only. Once only did a somewhat more severe bronchial affection exist for a short time. Milder or more severe attacks of the fever appear to affect its intensity just as little as its occurrence. In the two classes just mentioned, I. included 26 per cent., II. 29 per cent. Of course, serious affections of the lungs, resulting from the highly-developed catarrh and the fever, are here entirely wanting.

The state of the intestines is, in the various epidemics, so changeable, that it must not cause surprise if but few phenomena

of this kind were present in the cases observed by me. The epidemics of 1865 and 1868, from which the greater part of my materials was taken, were marked by the slight prevalence of bowel complications. Only 16 per cent. are recorded altogether. To the cases of ten days' duration belong 21 per cent., to the severer ones 12 per cent. From this side also we have a confirmation of the fact that the intensity of the disease stands in no constant relation to that in the intestine. *Tenderness on pressure* in the ileo-cæcal neighbourhood is scarcely ever wanting, and spontaneous pains in the abdomen were pretty frequently present. Perforations or hæmorrhages did not occur.

It would be a great mistake, however, to ignore the bowel affection. For the treatment, it possesses the chief importance. I shall speak of this circumstance later on, and therefore merely hint at it now.

In the urine we find *albumen* often and early, especially in the cases accompanied by high fever.

The *recovery* is always comparatively slow; it stands in no direct relation to the symptoms observed during the disease. Great languor, and, above all, *fatigue of the muscles*, of which forced use may produce a temporary inactivity, frequently occur. The mental functions also continue wanting in energy for a considerable time. The recovery is also threatened with *relapses*, at all events in its earlier part. Here a point already mentioned comes into full force. *Any one who commits an error of diet after mild typhoid may reckon with tolerable certainty upon again falling ill.* In hospital treatment we have naturally but little opportunity of observing this. It is just the reverse in private, and especially in out-patient practice. Here it not very unfrequently happens that, by the fault of the patient himself, or of his attendants, a mild case of typhoid is converted into a severe one.

Within the space of a few weeks I observed, in the out-patient department at Tübingen, four cases of typhus levissimus, of which no less than three had relapses after a normal temperature had been reached for one or more days. In one patient the disease dragged on, in consequence of repeated acts of imprudence, into the sixth week, although already on the

fourteenth day normal temperatures were taken morning and evening. The patient was thus brought to the edge of the grave, had to undergo a very severe attack of collapse, and was reduced almost to a skeleton. I have never seen relapses without some demonstrable error of diet. I am, therefore, much stricter with regard to diet, in the treatment of typhus levissimus, than in the case of the fully developed disease. No patient receives solid food until six days have passed after that on which four measurements, at least, taken in the rectum have failed to give a temperature as high as 100.4°. Even this limit is, perhaps, not sufficiently wide; but in a large majority of cases it suffices. *I am of opinion that* TYPHUS AMBULATORIUS *is nothing else than* TYPHUS LEVISSIMUS *prolonged by repeated errors of diet.* Without injudicious diet, the disease would quickly run a favourable course. The opposite procedure leads not unfrequently, as is well known, to a fatal result. On this account also the physician must make himself familiar with the abortive form of typhoid.

The fact that relapses so very frequently occur after errors of diet, indicates where we are to seek for the reason why, in spite of the introduction of a large quantity of the poison, a severe attack of the disease does not occur. I imagine that the intestine does not furnish a fitting soil for the increase of the substance which produces the disease, and it requires the stimulus given by another circumstance to render it capable of the reproduction. In my opinion, the intestine has claims to the chief part in the increase of the typhoid poison within the body; but it would lead me too far away from the subject before us if I were to enter further into that question. I will only add, that it is easier to understand the mildness of the disease, in spite of free infection, and the ready relapses after errors of diet, if we assume a *slight susceptibility of the intestine* for the typhoid poison, such as we may conceive in accordance with the analogy of the chancre poison developed above.

There is one more point of which I wish to speak. The *ætiological* moment is the determining one for the diagnosis of typhus levissimus. However closely they may appear to agree symptomatologically, unless we can demonstrate a common origin for the mild and severe forms, just doubts will always

remain. I have spoken already of the frequent occurrence of the mild form concurring with the severe form in *hospital epidemics*. This connection also is capable of a similar elucidation. I have determined from the case-books of the hospital in Kiel the distribution of the severe cases of typhoid over the individual months of the years 1851—1862. There results therefrom yearly, whether an extension of the endemic disease, an epidemic, existed or not, a distinct, constantly recurring rule. About 50 per cent of the cases fall within the months from August to November. The same applies to the mild typhoid, except that the occurrence of this form is more strictly confined to the *season, for 75 per cent. of the whole number of cases fall within the period mentioned.* The previous month, July, has, in reference to mild typhoid, 10 per cent., November about 6 per cent., of the annual cases, *so that for the whole remaining period of six months, only 9 per cent. remain.* In many cases, if an attack occurred out of the season, an especially intense cause of typhoid, a house epidemic with numerous severe forms, etc., could be shown. For its occurrence in the out-patient department the same holds good, but I am, unfortunately, not in a position to give exact dates and numbers there. I must also speak of the distribution of the cases in reference to age. In Kiel 89 per cent. of the cases of severe typhoid occur during the ages from 15—30; for the milder form I find about 94 per cent. That is to say, the disease designated by me as mild typhoid affects only that period of life which shows the greatest disposition to typhoid in general.

I believe that the *ensemble* of the arguments brought forward by me in favour of the existence of mild typhoid suffices to prove it thoroughly.

The treatment of the mild form may be expressed in a single sentence: rest in bed, strict diet, and antipyresis, when this is called for, by means of the cold water treatment or quinine. We should never let a patient suffering from typhus levissimus leave his bed in less than six days after his temperature had for the last time reached 104.4°; even so little should we allow him to take solid food at an earlier period.

In conclusion, I would ask my colleagues once more to

direct their full attention to these mild forms. I am justified in doing this, for I have found the dread of calling something "typhoid" which does not correspond to the orthodox picture is as strongly marked here in the south as in my old home. There, a physician made the distinction, simple enough it is true: if the patient dies, he died of typhoid; if he recovers, then he has had nervous fever. Was this mode of diagnosis beneficial for the sufferers?

# ON ARTIFICIAL PREMATURE LABOUR WITH A NARROW PELVIS.

BY

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IN MARBURG.

GENTLEMEN,—You have just seen a lying-in woman who has been delivered by artificial premature labour, and is now about to be discharged with her child. The woman had suffered from rickets for a long time in her youth, and had first learnt to walk in her fifth year. She has been delivered with difficulty three times at the full period by means of perforation, and each time made a very slow recovery. In her fourth pregnancy she came here for the purpose of having the Cæsarean section performed, since there seemed to be no other possibility of her very ardent wish to have a living child being fulfilled. I persuaded her, not without difficulty, to consent to the artificial induction of premature labour, and the result rewarded our choice. It was a foot presentation and the child was born asphyxiated after a difficult extraction of the head, but came to life in a bath, and was able to leave the hospital in good health in six weeks with the mother, who had remained perfectly well. Four years have passed since that time, and the child, which has thriven perfectly, is the source of constant pleasure to its parents.

The present, fifth pregnancy of this woman has proceeded no less favourably. This time also artificial premature labour has furnished a living child, the woman has made a good recovery, and both mother and child are in a condition to be sent home five weeks after the delivery without danger for the health of either.

Cases like the one before us are speaking examples in favour of artificial premature labour. If you bear in mind that in the case of our patient there is a generally very narrowed, flat pelvis, with an antero-posterior diameter of 2.96 inches, you will admit that the favourable result of the two last labours is not attributable to chance, but that artificial premature labour has here achieved a true success.

It is true, however, that this is not recognised everywhere. If you follow up the history of this operation, you will find that its results are often doubted, and by some entirely denied. From the very first period at which it was made known, artificial premature labour found many opponents, and fifty years passed in Germany before the decision was arrived at to adopt a procedure from the English which they had carried out with success. And as now, until towards the middle of the present century, the operation had gained more ground, opposition was again made to it, and urgent warnings given not to overestimate its value.

The opposition to artificial premature labour was kept up by the circumstance that the earlier materials for observation did not admit of a clear insight into the services rendered by this operation. In many cases, an exact diagnosis of the pelvis was wanting; in others, it was not stated whether the success was a permanent one for the child, and the opinion was thus formed that the operation was frequently undertaken unnecessarily, and that its unfavourable results were sometimes not recorded.

The idea of interrupting pregnancy before the full period in a woman with a narrow pelvis is so natural that a general justification thereof must appear unnecessary. Nature herself sometimes shows us this way, if a premature labour occurring spontaneously with a narrow pelvis gives a favourable result such as was not attained in previous labours at the full period. All the dangers which disproportion of space involve for mother or child will be less in the case of a small fœtus than in that of one born at the full time.

But in an operation such as the artificial induction of premature labour, the question of its admissibility must not be discussed on theoretical grounds only. The decision here rather depends upon experience, *i.e.* the careful examination and comparison of the cases previously observed.

This has been done in various ways by various writers. It is known that Scanzoni collected materials several years ago for confirming in a more general summing up his opinion concerning the over-appreciation of artificial premature labour. While we are still awaiting the appearance of this *résumé*, Spiegelberg has meanwhile preceded him with the publication of a considerable mass of evidence.* He stated that in 587 cases of spontaneous labour with a narrow pelvis, 6.4 per cent. of the mothers and 35 per cent. of the children died, while in 219 cases of artificial premature labour, 15 per cent. of the mothers and 66.9 per cent. of the children died. This result, Spiegelberg thought, spoke so strongly against the operation that we are not justified in undertaking it on the ground of narrowness of the pelvis alone.

Spiegelberg's statement increased at the time the number of the opponents of artificial premature labour, and through their influence cases of narrow pelvis which were known to me by experience remained without operation, the termination of which, if artificial premature labour had been induced, would, I believe, have been more favourable.

In reality, however, the confrontation made by Spiegelberg is not an appropriate one. If spontaneous labour with a narrow pelvis had given only 6.4 and 35 per cent. of deaths, artificial premature labour was not to be condemned because, in other cases of narrow pelvis, this method had given 15 and 66.9 per cent. of deaths. It is a question here of values which cannot properly be compared with each other.

A somewhat more favourable judgment may be formed from the collection of cases made by Litzmann.† That author grouped together the cases of narrow pelvis, according to the degree of the existing contraction, in four classes, and then examined how, in each of these classes, the results of the labours at the full time and of those occurring prematurely had stood in comparison to each other. He thus found that in the medium degrees of contraction of the pelvis, artificial premature labour endangered the lives of mother and child less than delivery at the full period. In the 47 cases which he designates as the second degree of contraction (simple flat and generally

* Archiv f. Gynäkol. Bd. i.
† Archiv f. Gynäkol. Bd. ii.

contracted flat pelves, with an antero-posterior diameter of 2.96—3.3 inches, and generally and uniformly contracted pelves, with an antero-posterior diameter of less than 3.6 inches), 81.2 per cent. of the mothers and 25 per cent. of the children were saved after delivery at the full time; 92.5 per cent of the mothers and 25.9 per cent. of the children, on the contrary, after artificial premature labour, whence he infers that narrowness of the pelvis alone justifies the induction of premature labour in such cases.

The gist of the question, however, is also not reached by Litzmann's collection of cases. The same objection which may be made to Spiegelberg's confrontation applies, though less forcibly, to Litzmann's. We certainly cannot arrive at a clear insight into the value of artificial premature labour if we compare deliveries at the full period and artificial premature labours occurring in different persons. The question in reference to which we must group the existing materials for inquiry must be put as follows:—

What was the result for mother and child when the labour commenced at the full period, and what was it for the same women when artificial premature labour was induced in them?

So only can the chances for the continuation of the pregnancy or for its interruption be correctly estimated. For such a classification the at present existing literature of the subject unfortunately furnishes no considerable material. If I thus classify the cases treated by me, we obtain the following result:

Of 32 cases of artificial premature labour which I have undertaken with a narrow pelvis,* 18 occurred in multiparæ. Amongst these women there had been 29 deliveries at the full period, which had been followed by bad recoveries in 13 cases (44.8 per cent.), while 26 of the children (89.6 per cent.) were born dead. On the other hand, the 18 artificial premature labours in these women gave 7 (38.8 per cent.) bad recoveries (with one death), and 8 (44.4 per cent.) dead children.

If I add to these numbers of mine also those artificial premature labours in multiparæ recorded in the literature accessible to me, which are said also to have occurred with contraction of

* I quote here, as in the following collection of cases, only such narrow pelves as belong to Litzmann's second and third degree of contraction.

the second and third degree,* I get a further review of 64 deliveries at the full period and 46 artificial premature labours occurring in the same women. The 64 deliveries at the full period gave (so far as the information extended) 68 per cent. of bad recoveries and 88 per cent. of dead children; the 46 artificial premature labours, 23 per cent. of bad recoveries (including five deaths) and 46 per cent. of dead children. The general result, therefore, was:

The 93 deliveries at the full period in women with pelvic contraction of the second and third degree gave 81 times, or in 87 per cent. of the cases, dead children, and, according to the information given for 48 of these cases, 26 times, or in 54 per cent., bad recoveries. On the other hand, 64 artificial premature labours in the same women gave only 29, or 45 per cent., dead children and 28 per cent. of bad recoveries, including 6 deaths. In other words, after the induction of premature labour in these cases, 42 per cent. more children were saved, and in 26 per cent. of the cases, the women were spared bad recoveries.

These numbers sound very differently to those of Spiegelberg, and they are large enough to encourage us in inducing premature labour in cases of narrow pelvis.

We conceive the indication for artificial premature labour to be as follows:

*If, in multiparæ with contracted pelvis of medium degree, we have ascertained from the course of previous labours that delivery at the full period endangers seriously mother or child, artificial premature labour is indicated and its induction justified by previous experience.*

The solution is thereby already pointed out for the majority of the cases in which such questions are presented to us, for multiparæ will always furnish the chief contingent for the artificial induction of premature labour for the reason already given, that our attention is often first called to the condition of the pelvis by the unfavourable termination of a previous confinement.

* The cases in question are taken from the publications of Credé (Monatschr. f. Geburtsk. Bd. xi.), Germann (ebendas. Bd. xiii.), Martin (ebendas. Bd. xix.), Künne and Berthold (Archiv f. Gynäkol. Bd. vi.), R. Greulich, d. Künstl. Frühgeb, Dissert. Berlin, 1872, M. v. Rutkowski, d. Künstl. Frühgeb,. Dissert. Breslau, 1872.

Are the primiparæ then, in whom the pelvis is contracted to a like extent, to be excluded from the operation? The idea has often found utterance, and, for the last time, in the Transactions of the Meeting of the Natural History Society at Wiesbaden in 1873.

If we find in the local misproportion the cause which justifies an early interruption of the pregnancy, we must recognise *a priori* the same necessity for primiparæ as for multiparæ.

That, in spite of this, many still advocate a different treatment for primiparæ and multiparæ, we still hear based upon the circumstance that the observation of a previous delivery renders a correct judgment of the local misproportion, the width of the pelvis and size of the child, the compression of the head, and the strength of the pains possible. These objections have been partly carried over from earlier times unfairly into the present day. With our present auxiliary means we are able, even in a primipara, to diagnose the width and form of the pelvis with the certainty necessary for treatment; and as regards a previous observation of the size of the child and the strength of the pains, the inference which we could draw from these data for a later delivery is a highly fallacious one. It is precisely in the case of primiparæ that we may expect that artificial premature labour will give us especially good results, because there many complications are absent which often spoil the result of the operation in multiparæ.* I have met with several cases of artificial premature labour terminating badly, in which I could only attribute the unfavourable result to the number of the previous pregnancies. I will describe such a case to you here.

Mrs. N., æt. 36, was ricketty as a child and did not learn to walk until she was six years old. She had been confined eight times when she was admitted into my clinique. In her first

* Repeated pregnancies bring with them one great disadvantage which I pointed out in accordance with Michaelis and in opposition to Litzmann, viz. decreasing strength of the labour pains. Out of 180 deliveries, sixty of which occurred respectively in first, second, and third pregnancies, I find weak pains recorded in 10 per cent. of the cases of first, 18 per cent. of the cases of second, and 28 per cent. of the cases of third pregnancies. In thirty-seven cases of fourth to eighth pregnancies weak labour pains occurred in 38 per cent.

labour she was delivered of a dead child with great difficulty by means of the forceps, and had a vesico-vaginal fistula which did not heal until six years afterwards. The second pregnancy terminated in abortion. The next five deliveries were all with instruments and furnished dead children. The eighth delivery was completed with the aid of the forceps, the child was again dead, and another vesico-vaginal fistula was formed which was afterwards cured by an operation.

In her ninth pregnancy the woman wished to have artificial premature labour induced.

On examination I found a simply flat pelvis, and estimated the antero-posterior diameter at 3.44 inches. The distance between the

| | | | |
|---|---|---|---|
| Spinæ il. amounted to | .. | .. | 9.8 inches. |
| Crist. il. „ | .. | .. | 12.4 „ |
| Conj. externa „ | .. | .. | 7.4 „ |
| Conj. diagon. „ | .. | .. | 4.1 „ |

The promontory stood deep and projected greatly, the side walls of the pelvis were difficult to examine with the exploring finger. On the extremities were found traces of the former rickets.

With this state of things, I could only assume that the pelvis would certainly admit of the passage of a premature child, and under otherwise favourable circumstances, of one at the full time, and considered artificial premature labour to be indicated according to my previous experience. What made me doubtful *a priori* of the success of the operation in this case, however, was the condition of the uterus. It had strikingly thin walls and surrounded the fœtus like a loose bag. The fœtus changed its position almost daily.

Since we could not calculate upon strong pains under these circumstances, I explained to the woman from the first that, in spite of the tolerably favourable proportions of the pelvis, the chance of a living child being born was not great. On the other hand, I believed that she might hope to escape the formation of another vesico-vaginal fistula, or the tearing asunder of the numerous cicatrices existing in the vagina.

At the end of the thirty-fourth week, just when I was about to introduce premature labour, a large quantity of the waters escaped spontaneously. Above the scarcely open cervix uteri I felt

the head of the child lying loose on the left side of the pelvis. Slight pains occurred from time to time. On the following day the breech presented. The escape of the waters continued and rendered the introduction of the inflating bag necessary. The pains were not rendered stronger thereby, although it was left in for some hours.

On the third day, notwithstanding the repeated employment of the upward douche, the introduction of a bougie into the uterus, and the administration of ergot, sufficient pains were still wanting, and in the evening, the waters having escaped entirely, the fœtal pulse ceased to beat, after which decomposition set in rapidly in the fœtus. The patient had a rigor and presented the symptoms of putrid infection.

Not until the following (fourth) day did the state of the os uteri admit of the removal of the greatly decomposed fœtus. The condition of the woman improved soon afterwards, and she was sent out well in three weeks.

The tenth labour of this woman ran a still more unfavourable course. A physician, who did not even live in the neighbourhood of the patient's home, undertook to induce artificial premature labour again in spite of my warnings. All I heard of the result was that both mother and child died during the delivery.

Our case shows the deleterious influence of repeated pregnancies, and you will acknowledge that if we can induce artificial premature labour in an earlier pregnancy, if possible in the first, the prospect of a successful result will be so much the better.

If we would investigate with ampler materials how far experience justifies us in inducing artificial premature labour in primiparæ, it will be necessary to compare the deliveries at the full period and artificial premature labours in primiparæ with each other in reference to their results. It is to be regretted that no one has yet undertaken such a classification on a large scale. My own experience concerning this question is too slight to be put forward as decisive, but I will not omit stating that fourteen artificial premature labours in primiparæ, with contracted pelvis of the second and third degree, seven times gave living children.

From what we know as yet, I believe *that we should make no*

*difference between primiparæ and multiparæ in our decision as to the induction of artificial premature labour, but that we should, in the one case as in the other, regard the existing contraction as an equally valid ground therefor.*

In simply flat pelves with an antero-posterior diameter of 2.8—3.2 inches, and in generally and uniformly contracted pelves with an antero-posterior diameter of 3.2—3.6 inches, the indication is clearest. The chances for a favourable issue of delivery at the full period are so doubtful that the interests of both mother and child are better served by an early interruption of the pregnancy. If the pelvic contraction is greater, only exceptionally favourable circumstances, *e.gr.* low pelvic walls, slight development of the fœtus, rigidity of the walls of the uterus, etc., would decide me to undertake artificial premature labour; if the contraction is slighter, I should prefer to wait for the coming on of labour at the proper time.

If you confine the indication within these limits, you will find that many cases of artificial premature labour described in books had better never have been undertaken. Even with a strict observation of them, we shall not fail to meet with severe cases, cases in which great demands are made upon our patience, our bodily endurance, and our manual dexterity. But, on the other hand, it would be a great mistake if we believed that we should improve our results if we subjected cases to artificial premature labour which might probably have had a favourable issue in labour at the full period. We must not forget that every artificial premature labour brings with it individual dangers for the patient which are absent in delivery at the full period, or are present in a slight degree only.

If we carry out the above principles, the period during which the induction of artificial premature labour is to be undertaken is also fixed. In a great majority of my cases I have operated in the thirty-fourth to the thirty-fifth week, and regard the result for the child as very doubtful if the degree of contraction of the pelvis compels us to undertake the operation two, or even three weeks earlier. It is unnecessary to remark that any earlier period brings our artificial procedure so much nearer to an artificial abortion. Even if we adhere in theory to the belief that already, at the end of the twenty-eighth week, extra-uterine viability begins for the fœtus, no obstetrician will really be

willing to undertake the operation at that time with any prospect of saving the life of the child.*

As already stated, the results hitherto attained by artificial premature labour fully suffice to secure a permanent place for this operation, on the sole ground of an existing contraction of the pelvis. But, at the same time, every one who inquires closely into the subject will be compelled to ask himself whether a further improvement of the results in the future is not possible. The numerous publications which have appeared during the last few years on the subject of artificial premature labour have, however various the views upon which they were based, had the great advantage certainly of having directed much more lively attention than was given before to the dangers connected with the operation. Every one who has induced premature labours during the last few years will, no doubt, admit to himself that he has neglected things attention to which only the most recent period has shown to be important for the result. I myself do not feel free therefrom, and rejoice to think that, by taking every precaution, I have recently attained better results than formerly.

In attempting to tell you what circumstances appear to me to be of especial importance, I must begin with the general remark that an artificial premature labour makes very different demands upon the continuous surveillance of the physician than any other case of labour. Such an operation often absorbs a great part of our time and energy; and I consider it highly dangerous to induce artificial premature labour in a place where we cannot be at hand at any moment if necessary.

The right understanding of this fact has already brought many cases of artificial premature labour into the obstetrical cliniques. But there, in the present state of many of our cliniques, the danger of puerperal infection for lying-in women is greater than elsewhere. Let it not be said that this circumstance is of equal weight in other confinements also. Artificial premature labours offer much more opportunity for infection than

* When such results are recorded, we may fairly be suspicious as to the correct calculation of the period of the pregnancy. Such accounts disagree with all our clinical experiences; and there is so great a want of exact data for determining the period of pregnancy, that the assumption of a mistake furnishes the best explanation of the majority of such cases.

others, and their completion without consequent puerperal disease is an excellent test for the healthy condition of a clinique. We hesitate to perform ovariotomy, or any other of the greater operations, when a hospital is not in a healthy condition; and we should also most strictly exclude cases of artificial premature labour from an unhealthy clinique.

The danger lies here in the long duration of the opening period, and in the repeated explorations which the watching of the case requires, and not less in the intra-uterine use of instruments which have already served for other purposes. Warned by previous results, I have been led never to entrust the examination of these cases to anyone but myself; and I should consider it unpardonable in the director of a clinique to allow students to watch these cases as they are allowed to watch all others.

Another circumstance which is also of great importance for the result is the most exact appreciation of the local misproportion. As regards the diagnosis of the pelvis, this requirement is sufficiently well known, but recent works have called our attention more than formerly to the other factor, the estimation of the size of the fœtus. We shall now no longer attempt to calculate this from the so often deceptive calculation of the period of pregnancy, but shall seek to form a more correct estimate of the size of the fœtus by means of repeated palpation, and, amongst other things, of Ahlfeld's cranio-breech measurement. I have, moreover, sought, in fitting cases, to push the head on to the pelvis, and then to ascertain from it, by internal examination, whether, and to what extent, a local misproportion existed between the head and the brim of the pelvis. Changes of the position of the fœtus also afford us some help here.

I regard a head presentation as the most favourable for the prognosis. This, it is true, is not admitted by all, every one judges here under the impression of his own special experience. If I appeal to my own, it speaks decidedly in favour of head presentations. In cases of transverse presentation, I prefer waiting with the induction of premature labour until a longitudinal position has been effected by the necessary manipulations. Experience has taught us, and Litzmann has given further valuable evidence of the fact,* that children born before

* Archiv. f. Gynäkol. Bd. ii.

the full time have a distinctly inferior power of resistance to complications which a well-developed child is capable of overcoming.

There is one other point upon which I must insist, even if the full discussion thereof is at present beyond my task, viz. the choice of the method. There is such a number of suggestions for the induction of premature labour that we see therefrom that a really good method has not yet been discovered. But more recent works have had the great advantage of showing more clearly than formerly the dangers of each method. In my opinion, however, we might make better use of the existing experiences than has yet been done. Methods of inducing artificial premature labour are employed by some which it would appear to me from our observations hitherto ought to be excluded.

Thus I have never been able rightly to understand why some practitioners have advocated perforation of the membranes without much distinction of cases. We know already from Hoffmann's statistics* that this method is highly dangerous for the child, and Schwartz's work on premature respiratory movements explains to us the nature of the danger. From the high position of the presenting part, which is in general the case in artificial premature labour, we have especially to fear the disadvantages of perforation of the membranes, and the certainty of that method for bringing on labour pains cannot counterbalance those disadvantages. I should only consider perforation of the membranes admissible in cases in which the presenting larger part is exceptionally low and firm upon the os uteri.

Still more decidedly, however, must the methods be considered as obsolete in which the danger of the admission of air into the uterus cannot be avoided. This applies, above all, to Cohen's method. With these intra-uterine injections, it is quite impossible to prevent with certainty the entrance of air, and if this does not happen it is purely a fortunate accident. We must no longer expose the patient to this danger, since many fatal results have been known to follow this method, and since we have become acquainted with the import of the rigors and sudden loss of power which often follow directly upon the employment of these injections.

* N. Zeitschr. f. Geb. Bd. xxiii.

I do not even consider the introduction of an ordinary elastic catheter with open ear high up into the uterus as admissible. Air will certainly be admitted into the uterus thereby, and however small the quantity of it may be, it suffices to set up putrid decomposition in the secretions. One patient died under my care who became infected in this manner in spite of the new catheter, and I have since made it a rule to use only bougies closed at the point. To the physician, it makes no difference whether he chooses the one instrument or the other, for the patient, health and life may depend thereon.

With such views I believe, Gentlemen, we must at the present day undertake a case of artificial premature labour. We shall feel convinced that in a case judiciously chosen, the chances of artificial premature labour are better than those of a confinement at the full period, but we shall none the less bear in mind that artificial premature labour requires of us a stricter surveillance than before.

If it prove successful, you will, in many cases, cause a domestic happiness which, without it, would be constantly longed for but never attained. The present case is an instance thereof.

ON THE

# TREATMENT OF DISEASES OF THE STOMACH.

BY

PROF. W. O. LEUBE,

IN JENA.

One of the acquirements of modern medicine is the conviction which is constantly gaining firmer ground in the circles of its representatives that physiology, more than any other natural-science training, is destined to furnish the scientifically-thinking physician with the clue to diagnosis and treatment. The close relation in which physiology stands to practical medicine is evident if we bear in mind that the manifestations of the organism when it has become diseased are connected immediately with the course of its functions when in a healthy state, and that a difference between them exists in *form* only. Consequently, discoveries in this department of the natural sciences also will, of themselves, be followed by consequences in pathology, as treatment also, dependent as it is and must remain upon empiricism, will always have to acknowledge its obligations to physiological principles, since its purpose can be no other than that of leading back abnormally-developed vital energies into the normal, *i.e.* physiological course.

I preface my lecture on the treatment of diseases of the stomach with these introductory remarks, because you will hear in it much physiological matter which may appear to some of you *a priori* not altogether compatible with the practical standpoint. But you will not, I think, fail to admit that the physiological points upon which I shall touch are always intended in my representation merely to form the test for the

correct interpretation of our experiences, and I hope to be able to show you that the employment of physiological facts may have immediate practical consequences.

The treatment of the diseases of the stomach coincides, to a great extent, with the treatment of *dyspepsia*.* In any case, the latter generally forms by far the most prominent of the symptoms which present themselves in the course of the individual diseases of the stomach. Whether the ailment treated be an acute superficial, or a more deeply-extending inflammation of the walls, whether an ulcer or cancer exist in them, or whether, without any ulcer or process of new-growth, chronic catarrh or dilatation of the stomach be present in the patient, the change in the structure of the walls of the stomach always shows itself by a change in the function of the organ, the chief expression of which are the well-known phenomena of "difficult digestion," the symptoms of "dyspepsia." It is the latter, therefore, which generally first come under consideration in the *treatment* of diseases of the stomach. We will first direct our attention to them, therefore, and not speak until later on, and then briefly only, of all the other indications which present themselves in the course of these diseases.

The signs which enable us to recognise the existence of dyspepsia at the bedside are, as is well known, the following: irregular appetite, from a slight perversion of the same, a liking for piquant or sour dishes, to complete loss of appetite, generally accompanied by increased thirst, an unpleasant taste in the mouth, a feeling of pressure in the epigastric region, the rising of gases or sour fluids into the mouth, and, in some cases, actual vomiting. These are accompanied by general weariness, hypochondriasis, headache—in short, various reactions of the nervous system upon the at first purely local disease of the stomach. It is not difficult to diagnose dyspepsia on the ground of the above-named symptoms. But easy as this latter is, just as difficult is it to find the proper remedy for this very fre-

* I would here state distinctly that I consider it wrong to separate *dyspepsia* as a special disease from the other diseases of the stomach, as has been done of late, and to treat of it apart from them. I rather understand by it a complexus of symptoms connected with the most varied diseases of the stomach, with the diagnosis of which nothing further is said than, for instance, with that of a dysmenorrhœa, etc.

quently-occurring complexus of signs of disease in an individual case. For, unfortunately, we possess no specific for dyspepsia like quinine for ague, or mercury for syphilis; every individual case of dyspepsia requires rather its own analysis, *i.e.* the determination of the question what kind of dyspepsia it is, and which of the various causes is at work in it. But these latter are naturally very numerous in a form of disease so frequent, as will become clear enough on closer inquiry.

The two constituents of normal gastric juice having a strong acid reaction are, a free *acid* and the hydrolytic, albumen-decomposing ferment, *pepsine*. Each of them is equally important for the process of digestion, one without the other not efficient; moreover, a certain proportion of both constituents is necessary for the production of active digestion. As regards the latter point, the most favourable proportion is 1—4 *parts of acid to* 1000 *parts of gastric juice*, while it seems to be much less important that pepsine should be present in any very definite quantity. Experiment has shown, indeed, that with an equal amount of acid, the solution of the albumen occurs so much the more quickly the richer the fluid is in pepsine, but, on the other hand, it has also been found that the smallest quantity of pepsine suffices for the solution of unlimited quantities of fibrin, if care is only taken that fresh acid is always at the disposition of the process of digestion; it appears from this that the pepsine remains unchanged in the act of the digestion of the albumen.

If we realise to ourselves at the bedside these physiological conditions connected with digestion in the stomach, especially the circumstance that the possibility or, at least, the rapidity of digestion depends upon the correct proportion of the two chief constituents of the gastric juice, we shall not be surprised if derangements of the action of the stomach are amongst the most frequent occurrences. They must occur so soon as the quantitative relation of those materials to each other is not the normal one, and their proper co-operation consequently interfered with for a time. It is clear that *too much* or *too little* of either of them may come into play in digestion, and make itself known by anomalies in the process, and it is equally clear that the only right treatment in such cases will be to improve what has become faulty in the composition of the gastric juice, either

by the artificial addition of acid or pepsine, as the case may be, or by neutralisation of the superabundant acid with carbonate of soda, or by precipitation of the excess of pepsine with one of the substances which produce that effect, *e.gr.* a metallic salt.

Starting from this principle, I adopt the following plan, in any case of dyspepsia of longer duration the cause of which is not otherwise easily ascertainable, to establish the diagnosis and the necessary treatment in an individual case.

I let the patient take about twenty-five grammes of Carlsbad salt on an empty stomach, so as to cause anything which may remain in it to pass downwards; then about noon some plain, cold roast veal, with or without bread. In from one to two hours afterwards, I take out a portion of the contents of the stomach with the stomach-sound, and convince myself concerning its smell and reaction, as well as how far the process of solution has advanced in the pieces of roast meat. I next put into three bottles equal quantities (about fifty cubic centimeters) of the filtered contents of the stomach, and hang in each of them a bundle of boiled fibrin of about equal volume. Into one of these bottles I put nothing further, into the second two drops of hydrochloric acid, and into the third two drops of a neutral solution of pepsine.* All three bottles are then placed in a large vessel of water, the temperature of which is kept at 95°—104° Fahr. The digestion which takes place in the two last bottles will show whether one or the other of the additions effects a more rapid solution of the fibrin than occurs in the first bottle, or whether they remain without import.

On the ground of the experiences I have hitherto acquired, and after simple consideration of the possibilities which present themselves, I cannot do otherwise than express my belief that, *in the majority of the cases which are accompanied by disturbances in the proportion of the constituents of the gastric juice, a deficiency of acid is to be regarded as the cause of the anomaly in the digestion.*

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As regards individual points in the changes in the *amount*

* I prepare this in the following simple manner, which does not, it is true, furnish a perfectly pure solution of pepsine, but yet one answering the proposed end: a piece of the mucous membrane of a pig's stomach is left for self-digestion with 5 per cent. phosphoric acid at 95° Fahr., and the resulting solution neutralised exactly with carbonate of soda.

*of pepsine*, it is not *a priori* very probable that an *abnormal increase* would give rise to disturbances of digestion, since an acceleration of the process of digestion accompanies the increase of the quantity of pepsine when the amount of acid remains the same. But just as little can a *decrease in the amount of pepsine* be followed by any other disturbance than a slight retardation of digestion. For since pepsine, as a ferment, only requires to be present in the smallest quantity to dissolve albuminous matters *ad infinitum* if fresh diluted acid is always present, it depends merely upon the extent of the secretion of acid whether the process of digestion shall be carried on continuously. Hitherto no case has come under my treatment in which the arrangement just described has shown that the bundle of fibrin to which pepsine was added had been dissolved rapidly. Generally only a very slight difference or none at all could be seen between the negative result in the bottle containing the mere contents of the stomach and that in the bottle which also contained the solution of pepsine. Very striking generally, on the other hand, is the result of the digestion in the bottle to which acid has been added; the bundle was entirely dissolved after 12—24 hours. In such cases, therefore, *the administration of an acid is indicated from a therapeutical point of view.* The acid must not, however, be given merely by routine—every two hours a tablespoonful of a mixture containing it—but so that the patient shall take his medicine immediately before and after a meal. I generally give eight drops of hydrochloric acid in half a wine-glass of water an hour after a meal, and repeat it in severe cases three hours later. I have always obtained good results from this treatment in the cases to which it is suited. It is interesting that Manassëin* quite recently, in his experimental studies of two pathological conditions produced in dogs, fever and acute anæmia, which he carried out under Hoppe's guidance, has shown that *a deficiency of acid* was also the cause of the deranged digestion existing in these diseases. On the strength of his experiments, he came to the conclusion that, "as well in an animal with fever as in one in which acute anæmia has been produced by bleeding, the normal proportion of acid and pepsine is changed, and that an addition of acid to

* Virchow's Archiv iv. s. 451-52 und Centralblatt für die medic. Wissenschaft, 1871. s. 853.

the gastric juice in such animals has, *cæteris paribus*, more effect than in healthy animals." The last possibility which may occur in the quantitative changes of acid and pepsine as causes of disease is *an excessive formation of acid*. However general the belief may be that the latter anomaly quite commonly exists in diseases of the stomach, I must distinctly express my doubts that the abnormally copious formation of acid interferes with digestion in many cases. I have no intention to deny that the matters vomited by many persons suffering from disease of the stomach have an intensely sour smell and reaction, but it is by no means proved thereby that, in such cases, really too much acid for the carrying out of active digestion in the stomach has been formed. Neither is an intensely "sour" *smell* which the contents of the stomach give off any proof of their intensely acid reaction, as I conclude from the fluid drawn up from the stomach in a case of dilatation now under my care, in which that fluid has a disagreeable, distinctly sour smell, but nevertheless reacts neutrally. But further, even when a strongly acid *reaction* has been verified in the contents of the stomach, the question is by no means answered whether too much acid is really present for digestion in that particular case, for the simple reason that we do not know therefrom *of what kind* the acid is which caused that reaction; and, on the other hand, a very important difference exists between the effect which one acid and that which another acid, in conjunction with pepsine, exerts upon the ingesta. Thus it has long been known that in pathological processes of fermentation in the stomach, sometimes lactic and later on butyric acid, but sometimes acetic acid also may be formed, and that the contents of the stomach then become strikingly acid. But these newly-formed acids, with the exception, perhaps, of lactic acid, are not capable of taking the place efficiently of the hydrochloric acid contained in the normal gastric juice. For even if they are adapted, like all other acids, for forming combinations with pepsine capable of effecting digestion, such combinations still digest feebly only, and it has been specially demonstrated experimentally in the case of acetic acid that it acts only very slowly when combined with pepsine, and that ten times more of that acid is required for active digestion than of hydrochloric acid. Moreover, I have myself already frequently observed that, in spite of a dis-

tinct acid reaction of the fluid brought up with the sound from a diseased stomach, an addition of hydrochloric acid to it in the test-glass evidently increased its solvent power. The following case out of my practice will serve as an instance of this: a schoolmaster has suffered for twenty years from heartburn and regular vomiting. I use the stomach-pump, and bring up a fluid from the stomach which gives the impression of being very sour, but which contains undigested portions of food five hours after the patient had taken food easy of digestion. In this case, the cause of the abnormally slow process of digestion appeared, at first sight, to be anything rather than a deficiency of acid, and yet it was shown on experiment that even under these circumstances, not a deficiency of pepsine, but a deficiency of hydrochloric acid was to be regarded as the cause of the bad digestion, since the addition of a slightly alkaline solution of pepsine to the pathological gastric fluid still further retarded the solution of albumen.

I believe that you will be inclined from this to look more favourably upon hydrochloric acid in the treatment of diseases of the stomach than has been customary of late years, during which all good effects have been expected from the administration of pepsine wine. The acid will appear especially indicated in convalescents, anæmic subjects, or persons suffering from feverish symptoms; also in cases of dilatation of the stomach which so obstinately resist all chemical means. I believe, from my experience hitherto, that hydrochloric acid will produce decidedly good effects. The observation of Manassëin, that matters in process of digestion with the natural gastric juice of the animals thrown into a state of disease by him, readily run into decomposition if no acid is added to them, may serve as a hint to give hydrochloric acid to patients in whose stomachs distinct processes of decomposition occur. Unfortunately, the number of my experiments with hydrochloric acid in the above described form of its employment in the specially mentioned cases of disease of the stomach is not so large as to enable me to define with the certainty which appears to me desirable the field in which it would always prove efficacious. But it will be more easy to determine the point in this country, with the great extension of diseases of the stomach in Thüringen, than anywhere else. I must, however, distinctly remark that with my

great predilection for hydrochloric acid, I by no means wish to assert that the use of pepsine is never indicated. I have only sought to make it plausible to you that in the majority of cases the deficiency of acid, and not a diminution of the quantity of pepsine in the gastric juice, is the original cause of the dyspepsia; on the other hand, the administration of pepsine as well as hydrochloric acid is necessary in the cases in which the latter given alone has not the desired effect, since it is well known that in such cases, with the same amount of acid, the digestive fluid acts the more quickly the greater the proportion of pepsine in it is. According to the most recent experiments by Panum,* the essence of pepsine prepared after Liebreich's prescription by the chemist Schering in Berlin is the most efficacious preparation; next in value is the French pepsine, which is a combination of pepsine, peptones, and lactic acid; so that, in this preparation, acid also is given with the ferment.

Cases which may be placed in the category with those just discussed are, in my opinion, those in which there is a copious formation of mucus in the stomach. By supplanting the normal production of acid, this gives rise to abnormal products of digestion. This hypothesis appears to me necessary to explain the otherwise insufficiently established assumption, that the presence of mucus in the stomach is the cause of the fermentation in it, the effect of which we observe in the formation of acetic, butyric, lactic, and carbonic acid, and of hydrogen. If the formation of these abnormal products of fermentation becomes too considerable, we are compelled in such cases to administer bicarbonate of soda, or some other antacid, from time to time. This indication is the more distinct, since the alkali not only neutralises the strong acids, but also, when given freely, excites normal secretion from the walls of the stomach.

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In addition to the changes in the mutual proportion of the two chief constituents of the gastric juice which we have just been considering, a change in the *quantity of the gastric juice secreted in toto* is, in many cases, to be regarded as the cause of the dyspepsia, although the proportion of the individual con-

* Centralblatt für medic. Wissenschaft, 1871. s. 805 (Referat) Om Tilveebringelsen of Mavesaft, etc.

stituents to each other may be the normal one. I will remind you first of all in this respect of the well-known experiments of Beaumont,* who ascertained by observation in the individual with an external opening into the stomach, upon whom he experimented, that, if he were suffering from fever, no more gastric juice was secreted, even on the stimulus of food taken. The latter remained 24—28 hours unchanged in the stomach. Also, in the so-called "*torpid weakness of digestion*," the stomach is no longer capable of corresponding to the usual stimulus of food with sufficient secretion of gastric juice. Persons suffering from this form of dyspepsia, therefore, seek to remedy the want of sensibility in the mucous membrane of their stomachs by the use of strong spices, or other means of stimulating it. The direct proof of the existence of such pathological conditions may be furnished in the following way:

If we introduce the sound into the empty stomach of a healthy individual, the mechanical stimulus, in combination with that of the cold water poured in with it, suffices to cause a slight secretion from the mucous membrane, in consequence of which a fluid with feebly acid reaction flows from the end of the sound. But this very slight stimulus evidently no longer suffices in those conditions of torpid weakness of digestion to incite the glands to secrete. I know a man who suffers from difficulties of digestion, from whose stomach I never drew up any but a neutral fluid by the procedure just mentioned; and within the last week only, I have drawn up fluid with a neutral reaction, on washing out the stomach of a lady whom I am treating for dilatation of that organ. Such cases of an entire want of secretion of gastric juice are treated with rhubarb, ipecacuanha, etc. I am of opinion that, with this torpor of the mucous membrane of the stomach, less confidence should be placed in the action of those drugs than in very gradually accustoming the stomach to do without such powerful stimulants. The carrying out of this latter plan naturally calls for great patience on the part of both physician and patient. In cases in which more speedy help is necessary, it would be advisable to give remedies which our physiological experience tells us are capable of causing a more copious secretion of

* Beaumont, W., Experiments and Observations on the Gastric Juice, etc. Edinburgh, 1838, p. 99.

gastric juice from the mucous membrane of the stomach. We possess such means in ether, alcohol, *cold water*, the *alkalies*, and very probably in bitter remedies. Amongst the alkaline natural waters, those of Vichy, Bilin, etc., would probably prove of use. The treatment may be aided by giving ice and bitters, the action of which, as I must assume from my earlier experiments (Rostocker Naturforschervers. 1871. Tagbl. s. 112), is of a purely reflex character, perhaps dependent solely upon the copious secretion of saliva, which, when swallowed, furnishes a powerful stimulant for the secretion from the mucous membrane of the stomach.

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The product of the action of pepto-hydrochloric acid upon albuminous matters, and upon tissues which furnish gelatine, are *peptones*, which represent, as is well known, albuminous matters in an altered state, chemically as well as physically. These are so far an important link in the process of digestion, as, in consequence of their low osmotic equivalent, they are absorbed about seventy times more easily by the walls of the stomach than ordinary albuminous matters. This absorption of the peptones formed in the stomach is, of course, of the greatest importance for the nutrition of the individual; but, quite apart therefrom, their constant absorption is a necessary process for a thriving condition of the body, for numerous experiments have shown that, in artificial digestive combinations, the albuminates only become thoroughly peptonised when the prepared peptones have been disposed of by dialysis. *The accumulation of peptones in the stomach is, therefore, an obstacle to the continuous solution of the still undissolved albuminous matters.* In proportion as the power of absorption of the mucous membrane of the stomach decreases, so is the course of the process of peptonisation in the stomach interrupted, the natural consequence of which are disturbances of the process of digestion. This explains Ranke's experience that, of one and the same quantity of meat, more is digested if it is divided into smaller portions than if the whole is eaten at one meal. But various clinical experiences are explainable in the same way. If we start from the assumption that deficient absorption of peptones may be a cause of dyspepsia, we may fairly attribute a part of the derangements

of digestion in chronic catarrh of the stomach to a diminution of the power of absorption in its mucous membrane.

If a retardation of absorption is a phenomenon to be observed daily at the bedside in chronic inflammations of other organs, so has a considerable change in the walls of the vessels, in the chronic form of this pathological condition, also been rendered highly probable by the most recent investigations. But the effect of this fault in the process of digestion, *i.e.* of a deficient power of absorption in the mucous membrane of the stomach, becomes very especially evident, in my opinion, in cases of *dilatation* of the stomach. If the absorption of peptones by the mucous membrane of the stomach has become limited, the already digested portions of food will collect in it, together with the undigested portions, and if this state of things go on for any considerable time, will gradually cause an enlargement of the cavity of the organ. Such an accumulation is evidently very especially favoured in cases in which mechanical impediments, *e.gr.* a cicatricial stricture resulting from an ulcer in the neighbourhood of the pylorus, exist to the exit of the contents of the stomach. But it is not possible to refer the whole complexus of symptoms, in cases of dilatation of the stomach, to contraction in the neighbourhood of the pylorus, because the degree of dilatation of the organ, observed after death, is not unfrequently found to be much more considerable than might have been expected with the degree of contraction at the pylorus, a state of things which may easily be made to accord with the symptoms during life, if that deficient power of absorption in the mucous membrane of the stomach is brought in aid of the explanation. By the assumption of the last-named anomaly of function, it becomes more easy than has hitherto been the case (cf. Kussmaul, Deutsch. Arch. für klin. Med. vi. 470) to explain why, in cases of dilatation of the stomach, that organ is in a short time rendered capable, by the use of the stomach-pump, of expelling the food through the contracted pylorus. From our point of view, the stomach, in the cases here described, has again begun to absorb, after the frequently enormous mass of unabsorbed fluids lying in it has been removed several times in succession. A part of the burden is thus removed which lay upon the muscular coats of the stomach during the whole course of the disease, the work of those coats

becomes easier, and they thus gradually re-acquire the power of expelling the contents of the organ. That in such conditions of deficient absorption of peptones, if they continue for any considerable time, the nutrition of the person so affected must suffer, requires no further elucidation, and many of you will have seen in your practice an instance of the frightful emaciation which finally occurs in the unfortunate individuals who suffer from long-continued dilatation of the stomach.

The actual existence of such a suspension of absorption in the stomach is demonstrable in a similar manner to the verification of the conditions of torpid weakness of digestion already described:

A patient whom my respected colleague Pfeiffer sent to me had been suffering for many years from disorders of the stomach, such as eructations, vomiting of large quantities of the contents of the stomach, accompanied by considerable emaciation, etc. If the patient laid himself on one side, the fluid in the stomach fell over with a loud noise on to the same side, and palpation showed distinct fluctuation; the patient himself was conscious how the fluid fell down when he drank anything; further, when I introduced the sound, it passed 3.2 inches downwards beyond the xiphoid process—in short, there could be no doubt that we had to deal with considerable dilatation of the stomach. In the course of the treatment, I introduced into the stomach a solution of meat containing peptones (see below), and did not allow the patient to take anything else for twenty-four hours. On the withdrawal of the contents of the stomach after that period, it was found that they did not differ in appearance from the solution of meat, etc., introduced at first, and that the amount of peptone in both fluids was about the same, whence it might be inferred that if an absorption of peptones took place in this case, it was, at all events, a very incomplete one. You also see from this case how an investigation with the sound may be turned to account as an aid to *diagnosis;* and the observation here described invites us, as it appears to me, to carry on further similar attempts to ascertain the cause of long-continuing anomalies of digestion, and thus to obtain a firm basis for diagnosis in difficult cases.

We must not overlook the fact, however, that much is not gained for the *treatment* of the disease by the verification of a

deficient power of absorption in the walls of the stomach, if we calculate the diagnostic and therapeutic gain only in as far as we have attained, by our search after the diagnosis, a *sure method* of treatment. For, unfortunately, we do not yet possess any remedy the employment of which offers us a fair prospect of exciting the power of absorption in the stomach. But a more correct formula for the diagnosis brings us, at least, this advantage, that it saves us from the useless administration of drugs. What I find to be the best thing to do at first is the regular washing out of the stomach. For this purpose I have generally used, for many years, not Kussmaul's stomach-pump, but a simple lifting sound.* With the latter instrument we avoid the danger of sucking in and tearing the mucous membrane of the stomach at the openings in the sound, of which accident Ziemssen has very recently published an instance. The decided advantage which the pump has over the various modifications of the lifting sound made known within the last few years is, that somewhat more solid portions of food can be withdrawn through the openings by the aid of the sucking power of the pump, which is not possible when fluids in the stomach are simply made to flow out through the lifting sound. But this deficiency may be partly remedied if care is taken in the choice of the lifting sound that whenever an interruption occurs to the flowing out of the fluid, water or air can be quickly driven through the choked sound to render it pervious again. The washing out of the stomach is, according to the experience of others besides me, an excellent remedy in cases of dilatation of the stomach. I candidly confess that I should not like to treat this disease, which is so common here in Thüringen, without the stomach-sound. I have often been convinced that all medicines, all regulation of the diet, all clinical and physiological calculation was useless in such cases, and that the use of the sound produced *at once* a tendency to improvement.

Together with the regular washing out of the stomach, a form of diet must be ordered which includes the smallest possible quantity of fluid. I let my patients take pieces of ice, or small portions of fruit ice, and advise them always to take their food in small quantities. In this way we may soonest expect to find the stomach again beginning to absorb small quantities

* Hegar's apparatus, which includes a glass funnel with a long tube.

of peptone. This treatment may be aided by hot poultices applied to the epigastrium, which prove also, in other parts of the body, to be the comparatively best means of preparing the way for more active absorption in deep-seated organs; I will remind you only, in this latter respect, of cases of chronic peritonitis and pleurisy. It is also to be recommended that the stomach, an organ become inefficient from the overwork of years, should, from time to time, have perfect rest, which may be best effected by the administration of nutritious clysters. That hydrochloric acid proves beneficial in many cases has already been mentioned.

I have tested the mode of treatment last described at the bedside, and am well aware that it is, above all, wanting in a means for directly and permanently increasing the power of absorption in the stomach. So long, however, as our knowledge of the process of absorption is so scanty as it actually is at present, so long can we scarcely hope to find the panacea for these states of disease.

*The derangements which occur in the function of the muscular coats of the stomach* are to be regarded as a further cause of the commencement and continuance of diseases of that organ. Apart therefrom that the chyme is expelled from the stomach by the contractions of its muscular coat, the movements which are caused by the regular drawing together of the walls of the stomach most probably serve to bring its contents into repeated close contact with the mucous membrane, to impregnate them with gastric juice, and thus to bring them into a state of solution. The repeated passage of the food over certain parts of the mucous membrane serves, at the same time, to excite the latter to secrete. Moreover, the muscular movement evidently favours the formation and absorption of peptones. Attention has also very properly been called to the circumstance that if the portions of food lie quietly in the stomach, the peptones formed from them accumulate at the point of formation, *i.e.* at the periphery of each morsel; and the further peptonisation of the albuminates in those morsels is arrested. And it may further be assumed that, through the movement to and fro of the food in the stomach, that which we attain by shaking an artificial digestive mixture in the test-glass is attained in a much higher degree, namely, a more rapid solution of the digestible portions.

I remark, in conclusion, that *the promotion of absorption* on the part of the walls of the stomach appears to me to be a not less important, though hitherto little noticed effect of the muscular movement. As in other parts of the body, so in the stomach also, the contraction of the muscular apparatus will furnish a powerful factor for the onward movement of lymph and venous blood, and therewith also for the continuous absorption of peptones.

Every impediment to the activity of this auxiliary mechanism, which we have seen to be so important for digestion by the stomach, will be followed by gaps in that process, and may lead to dyspepsia. Kussmaul has quite recently, in his well-known work on the employment of the stomach-pump, given closer attention to the state of the muscular apparatus in cases of dilatation of the stomach, and demonstrated, in the severe forms, the existence of a fatty and colloid transformation of the muscular cells. But also a simple weakness of the muscular apparatus, such as we assume to exist in chlorotic subjects and aged persons, a serous infiltration such as may occur in Bright's disease, in affections of the lungs and heart, or even in catarrh of the mucous membrane, suffices to invalidate the important aid for which the act of digestion is indebted to muscular contraction. In correspondence with our experiences and results in the treatment of paralysed states of other muscles, the employment of the electric current in these cases of weakness of the muscular apparatus would be advisable. The experiments made by me in that direction are, however, as yet too few in number to justify me in speaking of more than hopes and hypotheses. I only know that in many cases the current has a beneficial effect, and that there is no physiological reason why it should not act upon the muscular apparatus of the stomach through the walls of the abdomen.

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It is possible that certain forms of dyspepsia owe their origin to *nervous* influences. We know, at least, that in hypochondriasis, hysteria, affections of the organs of generation, cases of neuralgia, etc., dyspepsia occurs the production of which must at present be referred to reflex action. But since we as yet know next to nothing concerning the courses in which

the nerves of secretion run, it is easy to understand that the nature of "nervous" dyspepsia most readily eludes our comprehension, and it is advisable, in our explanations at the bedside of the occurrence of one or other of the forms of dyspepsia, especially to avoid as far as possible having recourse to the nervous system.

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I have hitherto intentionally spoken only of the derangements in the secretions of the normal constituents of the gastric juice and of the changes in the absorption of the products of digestion resulting therefrom, without alluding to the more deeply-seated and for the most part permanent changes in the coats of the stomach. I did so because, in the forms of disease of the stomach well marked in a pathologico-anatomical respect, such as ulcer, carcinoma, etc., the symptoms include without exception also the group of symptoms which I have just been attempting to portray and analyse as dyspepsia, and of which the treatment forms the most important and most general problem in the therapeutics of diseases of the stomach. It is not my intention to give you in the present lecture a detailed account of the other modes of treatment of diseases of the stomach with various drugs, etc., which are in use besides those I have already spoken of. You find in every manual what narcotics are given in cases of cardialgia, what antidotes in inflammations of the stomach resulting from poisons; further, that in cases of hæmorrhage from the stomach, the employment of styptics, cold, etc., is the clearest indication. I will here mention cursorily only that a course of natural mineral waters is justly recommended for diseases of the stomach, for nobody can doubt that many such patients owe their improvement or cure solely to Carlsbad, Ems, etc. But I must refrain from attempting to explain here the fine shades in the picture of these diseases which more or less clearly point out the value of one watering-place or the other. So much appears to me certain that statistics and experiment must contribute largely to furnishing with safe rules the physician who has to advise his patient to try one source or another. It is certain, however, that in many cases too much benefit is expected from the effect of a course of mineral waters, since the latter often plays the part of a last

resource for many patients and physicians, and is only resorted to in the most obstinate cases.

I wish, on the other hand, to speak more in detail of a subject which comes so essentially under consideration in the treatment of every disease of the stomach that even the most exact observation of the special indications mentioned above is successful only if, at the same time, minute instructions are given and absolute obedience required in this other respect, I mean in the *diet of persons suffering from diseases of the stomach.*

In laying down precise rules for diet, there is one, I think, which must take precedence of all the others, namely, *that for a diseased stomach no better diet exists than rest.* Let us recall how anxiously even the slightest movement is avoided in inflammation of other parts of the body, how, when recovery has commenced, we allow the organ in question only gradually to re-assume its function, and how successful, in most cases, is the enjoining of absolute silence in cases of inflammation of the mucous membrane of the larynx! Unfortunately we are scarcely ever able, in affections of internal organs, to keep them for any length of time in an entirely passive state. But precisely in affections of the stomach it is possible, by a suitable choice of the method of giving nourishment, to afford the diseased organ perfect rest or, at least, to reduce its work to a minimum.

*Perfect rest* can be attained only if the nourishment is not given in the usual way by the mouth, but per anum. Since it has been proved both by physiological experiment and by results at the bedside that, by the employment of the meat-pancreatic clysters recommended and used successfully by me* (Deutsch. Archiv f. klin. Medic. Bd. xi. 1 ff.), a considerable quantity of nitrogen can be introduced into the body, and a human being supported in this manner for a long time, that, moreover, the feeling of hunger is allayed by this mode of nourishment, the method will be indicated in the most severe forms of disease of the stomach. It thus becomes possible, without damage to the general nutrition of the patient, to save the walls of the stomach for days from the stimulus of food, so that

* Leube's solution of meat treated with pancreatine. Compare the most recent publications of Heyfelder (Deutsche Zeitschrift für Chir. ii. 3, s. 324) and of H. van Holsbeek (la Charité sur les champs de bataille, vol. viii. p. 170).

the morbid process in the mucous membrane may come to a natural end. This method has the further advantage that the remedies which are intended to act upon the internal surface of the stomach can do so much more energetically than if, as is usually the case, they become mixed with larger or smaller quantities of food, and thus come in a diluted or chemically changed state into contact with the diseased mucous membrane.

In most of the cases of disease of the stomach, however, it is not at all necessary to throw the diseased organ entirely out of work; we rather attain our object completely if we order for our patient "easily digestible" food, taking care, however, that the diet shall really be easily digestible. This brings us to the question which forces itself upon our notice daily at the bedside —a question as important as it is difficult to answer, viz.: *What diet corresponds to the notion of "easy digestibility."* Let us honestly admit that this question has often puzzled us extremely, because we feel, on the one hand, that the main point of the whole treatment of a given disease of the stomach lies in answering it correctly, and, on the other hand, that we cannot entirely suppress our own doubts as to the suitability of our choice if we order this or that diet as easy of digestion. We need not, however, be ashamed of this doubt, for really convincing experiments in this branch of physiology have not been made on a large scale. About a century ago, it is true, Gosse, who possessed the peculiar capability of bringing up the contents of his stomach at will by swallowing air, made very interesting experiments concerning the changes occurring in the food in the stomach at various intervals, and the result thereof, a statement of regular "classes of digestibility," is contained in the appendix to the celebrated work of the immortal Spallanzani on the theory of digestion. Beaumont also experimented in the same direction with the stomach of his Canadian, inasmuch as he determined the exact time which each article of food required to leave the stomach. But with all these experimental results, much is not determined concerning the digestibility of the various articles of food, as has been very justly pointed out by Frerichs, Kühne, and others. According to the observations of Busch and Kühne, almost undigested morsels of meat and albumen, as well as uncoagulated milk,

issued ¼—½ an hour after a meal from a fistula of the small intestine, whence it appeared that especial regard ought to be paid, in experiments as to the digestibility of the food in the stomach, to these apparently regular, very early evacuations of undigested portions of food from it. I am of opinion, however, that the question of the digestibility of various kinds of food cannot be clearly answered by exclusively or even chiefly taking into consideration the action of the gastric juice upon the ingesta generally. If we realise to ourselves the recent experiences concerning the rapidity with which the articles of food pass the upper portions of the alimentary canal (such as was observed in one of Radziejewski's* experiments, in which broken calves' bones swallowed by a dog with a fistula in the ascending colon appeared at the opening as early as eighty minutes afterwards), we shall, on the one hand, find Bernard's view, that the function of the stomach is chiefly preparatory to digestion, justified, and, on the other hand, be more and more forced to believe that a great part of the work of digestion is performed in the commencement of the large intestine. The power of absorption of this part of the alimentary canal is also placed clearly beyond all doubt by the experiments on the digestion of the meat-pancreatic clysters. The point of view from which we must start in our estimation of the digestibility of certain articles of food is thus shifted; it will be of less importance to determine in how changed a condition the food leaves the stomach than to ascertain how much changed it appears in the large intestine and finally in the excreta; how far, during its course through the *whole* alimentary canal, the fat, nitrogenous matters, etc., are actually extracted from it. A thorough extraction of those elements of the food *in the stomach* is by no means a very desirable event for a patient suffering from disease of that organ, inasmuch as the very articles of food which become little changed in the stomach and quickly pass out of it, if they do not injure the mucous membrane in a rough mechanical manner by the nature of their surface, consistency, etc., will incommode the diseased organ much less than the articles of food which are especially accessible to the solvent action of the gastric juice, but remain a longer

* Radziejewski, zur physiol. Wirkung der Abführmittel. Dubois-Reichert's Archiv 1870. s. 1, ff.

time in the stomach and make disproportionate calls upon its energy, already impaired by pathological conditions. Lastly, in a given case, the nature of the stomach affection and the individuality and constitution of the patient must be taken into consideration in connection with the question of the easy digestibility of the diet.

You will have gleaned from what has already been said the conviction that, from the scientific, physiological point of view, the question of the digestibility of individual articles of food is, on the whole, still an open one. We are not justified on that account, however, in not giving our patients any directions as to the choice of their food. Just as, in therapeutics generally, we must be led by empiricism as well as by physiological reasoning, and the latter is only meant to regulate scientifically the dogmas of the former, so ought we, I think, to be guided by experience in reference to dietetics, especially because unlimited materials from all times and nations are at our disposal here, and we ourselves are in a position to make our observations daily.

In this sense I do not hesitate to assert that, of the different *kinds of flesh*, young veal, fowls, and pigeons, are those least difficult of digestion. For many persons fish and beef will have to be included in this bill of fare, and it is advisable to allow the patient to take the former boiled only, while beef should always be eaten lightly roasted. It is an old experience in cooking, that meat much roasted or boiled becomes tough, and in accordance with this, the very recently published experiments, by Fick, have shown directly that one and the same gastric juice dissolves boiled meat three times more slowly than raw meat. In using roast meat, it is further customary, even amongst the laity, if easy digestion is kept in view (apart from sufficient mastication), to refrain from the use of rich sauces—a measure the importance of which consists in the fact that pieces of meat which are surrounded with fat become less easily penetrated by the gastric juice than lean pieces. Gosse has already called attention to this circumstance, in the classification of the results of his experiments mentioned above.

To the "easily digestible" of articles of food belong further, as proved by the experience of a thousand years, *milk* and *eggs*.

That milk, the most natural of all kinds of food, is especially well borne by many persons suffering from disease of the stomach, is an old observation familiar to you all, and *raw* eggs are also regarded as little burdensome to the stomach. From experiments of my own, however, I am inclined to doubt the latter notion (Tagbl. der Rostocker Naturvorschervers. s. 112). Boiled white of egg, on the contrary, appeared to me more easily digestible than in the raw state, and Fick's last statements prove, at least, that raw albumen has no advantage over coagulated albumen as regards digestion. I, therefore, never allow my patients to take their eggs otherwise than *lightly boiled.*

Of *vegetables* I allow only the tenderest kinds. Those most to be recommended are: asparagus, young hops, shelled sugar peas, and young carrots. Potatoes should never be taken otherwise than mashed. On the other hand, gruel made with milk and groats is especially to be avoided, since many of the groats appear to have such sharp corners as to be capable of irritating greatly the mucous membrane of the stomach, *e.gr.* of a patient having an ulcer in the stomach, of which I myself have recently seen a very striking instance.

Of *bread* only the white kind should be used, and that stale, because the digestive fluids are more easily taken up by it than by lumpy, freshly-baked bread.

*Spirituous liquors* I avoid, if possible, altogether in diseases of the stomach. If the use of wine is strongly indicated, it may be administered to the patient just as well per anum, as is very customary in England, as by the mouth.

It is not my intention to offer you further and more minute suggestions concerning the diet of your patients. What has already been said will suffice for ordinary cases, but this bill of fare must, of course, be modified from time to time.

If the observance of the diet just mentioned gives good therapeutic results in many cases of disease of the stomach, there are yet still more cases in which you will not attain your object therewith, since the patient cannot bear even this "easily digestible" food; and it becomes a question, what diet must now be employed? what diet is still more easy of digestion? It is evident that we should have attained the object of our wishes in this respect, if we had at our disposal a kind of food

which, while containing the greatest possible amount of nutritive matter, required the least possible amount of work, on the part of the diseased stomach, for its working up by and assimilation with the digestive fluids. This requirement would be best fulfilled by the *peptones* themselves which are formed in the stomach from the albuminous matters introduced into it and easily absorbed. But great technical difficulties have hitherto been present for their artificial production, since, in addition to the combination of pepsine and acid, a constant temperature of 104° for some days is required. Moreover, according to my pretty large experience in this respect, all solutions made with natural ferment have such a repulsive smell of vomited matter, that I could not expect any patient to swallow messes so prepared. I have, therefore, been endeavouring for about a year, in concert with my friend, J. Rosenthal, in Erlangen, by means of strong heat and acid, in air-tight vessels, to effect the same peptonisation in meat externally to the stomach which is effected in the human stomach, by means of pepsine, at a low temperature. The meat so treated falls, under the action of the two agencies just mentioned, into a very thin emulsion, and the solution contains a greater or less quantity of peptones. While the amount of peptone may differ, according to the mode of preparation, I believe, from my numerous experiments at the bedside, that the preparation brought some months ago into commerce, under the name of "*solution of meat*," is the least irritating and most nutritious form of meat known to me, and is extremely well borne, even in cases of the most obstinate vomiting. Its advantages over other kinds of nourishment employed in diseases of the stomach may be gathered from the following. They are:

1. *Its soft consistency and emulsion-like character.* Since the meat-solution forms a mass resembling fine mud in softness, it is clear that the mechanical irritation which other articles of food occasion in the mucous membrane of the stomach, by rubbing against its walls, is entirely absent when this solution is employed. This mud-like character of the preparation will evidently be especially suitable in cases of ulcer of the stomach, since all rough contact with the surface of the ulcer is thus avoided. And it is actually found in practice, that the attacks

of cardialgia cease from the day on which a patient with ulcer of the stomach begins to use the meat-solution persistently. I have had many instances of this, both in clinical and private practice. My mode of proceeding in the treatment of this affection is as follows: I keep the patient for 2—3 weeks in bed, and have during the day hot poultices, during the night one of Priessnitz's applications, kept on in the neighbourhood of the epigastrium, and allow him for a similar period to take no other food than a pot of the meat-solution (equal to half a pound of beef) daily, and two cups of milk, with finely powdered biscuit soaked in it. With this diet the patients recover remarkably quickly, with a certain uniform regularity, the vomiting ceases, the pain disappears, and even firm pressure in the neighbourhood of the epigastrium causes less pain from day to day. After 2—3 weeks, I cautiously begin to give more consistent food, with the assumption that the healing process in the ulcer is now sufficiently advanced. We must even now, of course, only expect from the diseased stomach the working up of the articles of food mentioned above as having been proved to be easy of digestion.

2. *Palatability of the preparation:* It reminds one by its taste of Liebig's extract of meat, but the solution is much more insipid than the latter. I either give it pure after it has stood in ice, or mixed with beef-tea, or I add a little Liebig's extract to improve the flavour. Judging from a number of cases which has gradually become large, I unhesitatingly assert that patients take the meat-solution very willingly, but will not attempt to deny that in many cases, after long use of it, patients begin to complain that it has become repugnant, etc. Let us honestly admit, however, that this is not the fault of the preparation, but of our habit of introducing changes into our food. Who would not feel repugnance, for instance, at eating boiled beef morning, noon, and evening for weeks together? I have had patients, however, who met the long use of it with complete indifference, and some again who consumed it with avidity.

3. *The great nutritious value of the meat-solution* is evident from the fact that all the component parts of the meat, all the nitrogen, salts, etc., are contained in it. A comparison cannot

be drawn, therefore, between it and Liebig's extract of meat, the direct nutritious value of which nobody will now place very high.

4. *The easy digestibility of the preparation* cannot, *a priori*, be doubted if we bear in mind the mud-like consistency and the treatment preparatory to digestion of the meat at a high temperature and with hydrochloric acid. But it is clear to me that points three and four, evident as they appear to be, cannot be determined scientifically until physiological experiments upon animals in the laboratory, and quantitative estimates of the amount of nitrogen in the excreta of persons living entirely upon solution of meat, have been made in convincing numbers, experiments with the performance of which we are at present occupied.

5. *The durability* of the preparation is ensured if sufficient care is taken in the preparation of it.* When in use, I keep the pot constantly in iced water to prevent the solution from spoiling.

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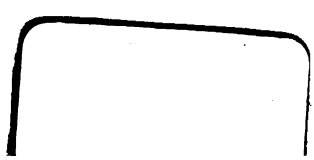
The field of the diseases in which the meat solution may be used with advantage is, of course, not the narrow one of affections of the stomach alone. In all the cases in which we are desirous of furnishing the organs of digestion with food which is absolutely unirritating, in which there is danger of overwork of the alimentary canal, or of mechanical irritation of its walls by the ingesta, the use of the meat-solution will be advisable. I will remind you of typhoid, dysentery, tubercular ulceration of the intestines, peritonitis, hæmorrhage from the stomach or intestines, etc., although, in the latter, nutrition may, of course, be carried on with less danger by means of meat-pancreatic clysters per anum.

I conclude with the hope that the results which my colleagues attain in their practice with the use of this preparation, whether unfavourable or otherwise, may be published. It has

* I have, unfortunately, often found that ill-prepared meat-solutions come into use at the bedside. I can only recommend provisionally, therefore, the preparations of the court apothecary, Dr. Mirus, in Jena, as free from objection.

come into use in an extremely short time in the whole of Germany, as well as abroad, as is proved by the innumerable orders given for it to the apothecaries here. Unfortunately, no printed reports as to its efficacy have yet been published. And yet it is only by the comparing of individual experiences with each other that the clinical value of the solution and the extent to which we should employ this new curative and nutritive preparation, which I believe destined to play an important part in the treatment of diseases of the abdomen, can be correctly estimated.

# INDEX.


Acid, carbolic, in treatment of wounds, 70
„ salicylic, use of, as an antiseptic 82
Ackerman (Prof. Th.), on the action of digitalis - - - - - 179
Albuminuria in diphtheria - - 420
Amputation-wounds, treatment of, 65
Anæmia, degrees of, and their treatment by auto-transfusion, etc. - 252
Antiseptic treatment of wounds, on the - - - - - - 63—168
Aphasia, syphilitic - - - 391
Atropine, influence of, on the pulse 184
Auto-transfusion, on - - - 237
Bacteria in sepsis - - - 64
„ presence of, in acute infectious diseases - - - - - - 16
Brain, syphilitic diseases of the - 377
Carbolic acid in treatment of wounds, 70
Chorea from neuritis - - - 234
Contagion, agencies of, on the - 16
Cord, spinal, chronic affections of, consequent on primary neuritis - 230
Coryza, acute splenic tumour with 9
Croup and diphtheria - - - 415
Cynanche contagiosa - - - 403
Digitalis, action of - - - 179
Diphtheria, acute splenic tumour in 6
„ lecture on - - - - - 403
Dohrn (Prof.), on artificial premature labour with a narrow pelvis - 473
Dysmenorrhœa, on - - - 47
Dyspepsia, on the treatment of - 486
Embolism of lung, symptoms of - 266
Epilepsy, secondary, due to neuritis, 232
Erysipelas, acute splenic tumour in 8
Fever, acute splenic enlargement in 2
„ typhoid, on - - - - - 454
Food of infants, on the - - 346
Friedreich (Prof. N.), on acute splenic tumour in relation to acute infectious disease - - - - - - 1
Genital canals, female, exudations near the - - - - - - 169
Gerhardt (C.), on hæmorrhagic infarction of the lungs - - - 261
Gusserow (Prof. A.), on menstruation and dysmenorrhœa - - - 28
Hæmoptysis in pulmonary infarction, 271
Hæmorrhagic infarction of the lungs, 261
Herpes Zoster, neuritis causing - 208
Hospital gangrene, influence of atmosphere in causing - - - 64
Hysteria from neuritis - - 234
Infants, food of, on the - - 346
Infarction, hæmorrhagic, of the lungs, 261
Infectious disease, acute, relation of splenic tumour with - - 1
Jürgensen (Prof. Dr.), on the milder forms of abdominal typhus - 454
Kehrer (Prof. F. A.), on the food of infants - - - - - 346
Labour, artificial premature, with a narrow pelvis - - - - 473
Laryngeal, recurrent, paralysis of 315
Lesser (Dr. L.), on transfusion and auto-transfusion - - - 237
Leube (Prof. W. O.), on the treatment of diseases of the stomach - 486
Lister's treatment of wounds, on 63
Lücke's (Prof. A.) surgical diagnosis of tumours - - - - 283

Lungs, hæmorrhagic infarction of the, 261
Measles, acute splenic tumour in - 7
Menstruation, on - - - 28
Metritis, peri- and para-, on - 169
Micrococci in diphtheria - - 430
Milk, administration of mothers' and cows', to infants - - - 346
Muscles, atrophy of, after neuritis 219
Nerve, recurrent laryngeal, symptoms of paralysis of - - - - 315
Nerves, an increase of irritability of, in tetanus - - - - - 225
Neuritis, as a cause of tetanus - 201
„ causing chorea - - - 234
„ causing epilepsy - - - 232
„ causing hysteria - - - 234
„ its diagnosis and pathology - 201
Nothnagel (Prof. H.), on neuritis in relation to its diagnosis and pathology - - - - - 201
Paralyses, respiratory - - - 309
Paralysis in diphtheria, on - - 423
„ of recurrent laryngeal, symptoms of - - - - - - - 315
Pelvic inflammation in connection with female genital canals - - 169
Pelvis, artificial premature labour with a narrow - - - - 473
Pneumonia, acute splenic tumour in 11
Psychoses, reflected, following injuries to the head - - - - 235
Pulse, action of atropine on the - 184
Pulse, action of digitalis on the - 181
Pyæmia, influence of atmosphere in causing - - - - - - 64
Recurrent laryngeal, symptoms of paralysis of - - - - 315
Reigel (Frank), respiratory paralyses, 309
Respiratory paralyses - - - 309
Salicylic acid, use of, as an antiseptic, 82
„ — in treatment of wounds - 63
„ — dressing of wounds, directions for - - - - - - - 158
Scarlatina, acute splenic tumour in 7
Senator (Dr. H.), on diphtheria - 403
Sepsis, influence of atmosphere in causing - - - - - - 64
Spiegelberg (Prof. Otto), on exudations near female genital canals - 169
Spinal cord, chronic affections of, consequent on primary neuritis - 230
„ —, syphilitic diseases of the - 377
Splenic tumour, acute, in relation to acute infectious disease - - 1
Stomach, on the treatment of diseases of the - - - - - - 486
Suckling, prohibition of, in various diseases - - - - - 348
Synanche contagiosa - - 403
Syphilitic diseases of the brain and spinal cord - - - - 377
Tetanus, case of, analysis of local symptoms of - - - - - 201
„ increase in irritability of nerves in 225
Thiersch (Prof. C.), on the Lister treatment of wounds, and use of salicylic acid - - - - - 63
Thrombosis, venous, symptoms of 265
Tracheotomy in diphtheria - - 451
Transfusion, on - - - - 237
Tumours, diagnosis of - - 283
Typhoid fever, acute splenic enlargement in - - - - - - 2
„ —, lecture on the milder forms of, 454
Typhus, abdominal, lecture on milder forms of - - - - - 454
Uterine, peri-, affections - - 169
Uterus, stenosis of the cervix of, as cause of dysmenorrhœa - - - 55
Variola, acute splenic tumour in - 7
Veins, thrombosis of the, symptoms of, 265
Wounds, antiseptic treatment of, 63—168
„ history of treatment of - - 65
„ treatment of, by Lister's plan and salicylic acid - - - - 63
Wunderlich (Prof.), syphilitic diseases of the brain and spinal cord - 377